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AN
INQUIRY
INTO THE
Nature and Properties
OF THE
BLOOD,
AS EXISTENT
IN HEALTH AND DISEASE.

BY C. TURNER THACKRAH,

M. Roy. Col. Surg. and Lc. Soc. Aph.

“Est quodam prodire tenus, si non datur ultra.”—HORACE.

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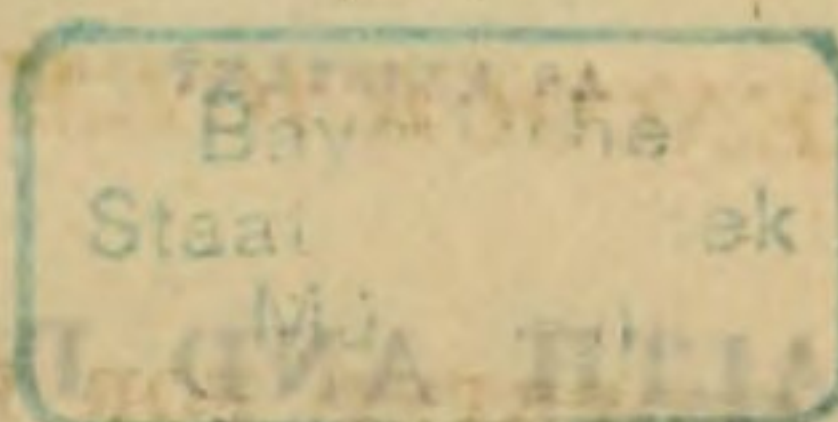
UNDER WHICH APPEARS

OF THE

THE ATTEMPT ORIGINATED

BLOOD

AND IN WHICH



IN HEALTH AND DISEASE

BY C. TURNER THACKRAH

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1870

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1870

TO

ASTLEY P. COOPER, ESQ. F. R. S. &c.

UNDER WHOSE AUSPICES

THE ATTEMPT ORIGINATED,

AND TO WHOSE

ENCOURAGEMENT

IT IS INDEBTED FOR PUBLICITY;

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THE AUTHOR

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TRINITY

success may have produced, the inquiry has demanded no ordinary degree of attention, perseverance, and labour. Nor will these qualities succeed in their object unless conjoined also with patience, caution, and candour. To the want of this combination, may be attributed the exuberant growth of those hypothetical notions which choke the spring of truth, and impede the progress of knowledge.

PREFACE.

MR. A. COOPER having offered to the gentlemen, educated at the School of Guy and St. Thomas, a Prize for the best Dissertation on the Blood, I instituted some experiments on the subject, and stated the results which my observations afforded. This Essay being so fortunate as to obtain the Prize, I have been induced to present it to the public. And, since, the late period at which I heard of Mr. Cooper's proposal, prevented the Inquiry's comprehending, in the first instance, some points of importance, I have, during the last year, been endeavouring to supply the deficiency.

In reference to the physiological parts of this Essay, it is scarcely necessary to remark, that much labour and considerable difficulty, attended the research. Whoever, indeed, has studied experimentally to elucidate the principles of the animal oeconomy, will admit, that, notwithstanding the interest it may have excited, or the gratification which

success may have produced, the inquiry has demanded no ordinary degree of attention, perseverance, and ardour. Nor will these qualities succeed in their object, unless conjoined also with patience, caution, and candour. To the want of this combination, may be attributed the exuberant growth of those hypothetical opinions which choke the spring of truth, and impede the progress of knowledge.

With all the efforts, however, and all the circumspection of the best-conducted inquiry, it is impossible to avoid the casual commixture of error, and the occasional assumption of doctrines, which subsequent examination proves erroneous. In physiological pursuits, accurate data are not afforded: demonstrative evidence, therefore, cannot be produced. The nature of the subject allows ample room, not only for experimental research, but for unbounded speculation, contradictory notions, and inconclusive statements. But these observations should by no means induce the opinion, that examination is fruitless, or truth undiscoverable. Such an inference is the refuge of ignorance and indolence. In the courts of justice, scarcely a cause is brought forwards, in which discordant evidence is not adduced; yet, who would thence conclude that a right decision is never formed, or truth never elicited? Not only in physiology, but in most inquiries to which the faculties of man are directed, conclusions are drawn, not with

mathematical certainty,—but from the balance of probability and improbability, by the contrast and examination of opposite statements, by reflection on the motives, principles, and objects of their authors,—and especially by personal scrutiny, closely and impartially conducted.

Of the principles and laws of the animal œconomy, few are more intricate than those which regard the Blood. The common appearances of this fluid may, indeed, be easily stated; but the origin of its changes, and the causes of its phenomena, are peculiarly abstruse; while the erroneous notions, and unfounded theories, which have been vainly adduced to remove the veil of nature, have greatly obstructed the path of inquiry, and added darkness to obscurity.

To clear away these obstacles, has been one object of my attention, and if in this only I have been successful, the science will be benefited. The labourer, who removes the rubbish on the scite of a projected building, raises not, indeed, the structure, but in preparing the way for the more able workman, he takes an office, though less respectable, yet not, perhaps, less useful.

Whatever opinion may be entertained of the success of my researches, of the mode in which they have been conducted,

or of the conclusions to which they have led,—I lay claim to fairness of intention and honesty of detail. Unbiased by prejudice, unshackled by preconceived notions, I have impartially stated the individual results of my experiments, and noticed every regular or casual discordancy. It has been my aim rather to ascertain facts than to support opinions ; to study the economy of nature, rather than to fetter her with conjectural or inconsistent theories. My inferences may be erroneous, but the facts remain unimpaired and unsophisticated. If they fail to substantiate the doctrines which they seem to support, they will not prove useless to future inquirers, and to science pursued under happier auspices. If the reasoning be rejected as futile or inconclusive, it may serve a minor, though not unimportant office—that of warning other experimentalists, of exhibiting the fallacies into which I have fallen, and by pointing out the ways of error, tend, in an indirect manner, to the advance of truth.

I trust, however, examination and experience will evince these inquiries to have added to the store of positive knowledge ; and if one doubtful fact be established, or physiological principle discovered, or practical point enforced,—I shall not have laboured in vain. The noblest works of art have been accomplished by small and reiterated efforts. The stately edifice is composed of single stones,

hewn by individuals, and the progress of science has been marked by no sudden bounds, but by the gradual and successive exertions of men devoted to the advance of the respective parts.

In reference to some of the experiments which were made on living animals, it may not be improper to make a remark; not so much, however, on account of the statements in the following pages, as in justification of those who have been thoughtlessly and ignorantly censured.—Wanton cruelty, professional men, I believe, universally abhor; the useless repetition of observations on living creatures, or the infliction of unnecessary suffering, they reprobate. And it gives me pleasure to state, that in none of my experiments did the animals seem to suffer more than from a natural death, *in few, so much*. But had my pursuits obliged me to violate my feelings, by the infliction of severe and continued pain, a laudable object would have ensured me the approbation of every friend of science and humanity. From experiments on living creatures, we derive our acquaintance with the functions of the animal œconomy. Were no such observations made, we should be ignorant of the offices performed by the several parts of the human frame, and consequently we should be unable to comprehend or remove its maladies. Surely then, while hecatombs are slaughtered for the gratification of our

palates, a few may be allowed to perish for the benefit of science: and while the luxurious complacently feed on the hare, which, half-dead in the chase, with fear and fatigue, has been worried by dogs,—or on the fish, which are burnt or scalded alive, they should not charge us with cruelty for occasionally sacrificing animals to the acquisition of that knowledge, which so eminently serves the cause of humanity, and prolongs the life of man. If they boast the fineness of their feelings, let them remember that we have feelings also;—if they be shocked at the thought of experiments on living creatures, let them consider what we suffer, not only in physiological researches, but in the ordinary offices of our profession, by stifling our natural emotions, and, from a sense of duty and humanity, coercing feelings as acute, perhaps, as theirs,—feelings, which certainly are not mitigated by the constraint under which they are laid.

The practical parts of this Essay, require little comment. Extensive opportunities have been afforded me of examining the Blood in disease, and of these, I have gladly availed myself. To state what observation and experiment have taught me, has been my principal object; not to form a compilation from the works of others, nor to canvass the justice of their notions, or the correctness of their remarks. The subject indeed, has necessarily led me to the occasional examination

of received opinions, but these were chiefly regarded as they influenced the treatment of disease: and if my observations have been frequently opposed to those of respectable writers,—it has ever been my aim, that important facts might be elicited, and safe indications in practice enforced.

The statements of the different sections are not partial and selected, but general conclusions from general results. I trust they will be found to assist us in discovering the nature of disease, in ascertaining its degree of violence, and in deciding on the treatment which it requires. Of this however, experience must form a judgment. They who apply to particular studies or endeavour to elucidate particular principles, generally overrate the importance of their pursuits: but the modest hope which I cherish, of having contributed, in some degree, to the stock of professional knowledge, will not, I trust, be found to have originated in vanity, rather than in truth.

C. T. T.

Leeds, July 1, 1819.

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AN INQUIRY, &c.

CHAP I.

GENERAL PROPERTIES OF THE BLOOD.

THE Blood is that peculiar fluid, which circulates in all classes of animated beings, and affords nutriment and vitality. It supplies the organs of Digestion, Secretion, Loco-motion, and Sensation; penetrating into almost every recess of the Body, and distributing itself through the numerous Capillaries of its Vessels.

The formation of the Blood is a process, which, like many others in the animal Œconomy, is imperfectly understood. That the Food passes through the Digestive Organs, undergoes certain changes, and forms a white fluid termed Chyle; that this substance is absorbed by the Lacteals, conveyed by them to the Thoracic Duct, and thence into the Venous system, is all that has been ascertained on the subject: but since no

traces of Chyle can be discovered after the Blood has passed through the Lungs, we presume that in these Organs, Sanguification is completed. It seems probable, however, that prior to its entering the Pulmonary Vessels, it assumes much of the sanguineous character; Chyle, when taken from the Lacteals, being found to separate into Serum and Coagulum, and to contain globules similar in form to the red ones of the Blood.

In discussing the general properties of the Blood, the first object which engages our attention, is the Circulation; but as this is not the immediate object of the present enquiry, it is unnecessary to detail a process so generally understood. Suffice it to notice, that the Heart, by its contraction, gives the primary impulse, the Arteries transmit the moving column, and the Veins return to the Heart all which has not been expended on the various structures.

When propelled from the Left Ventricle into the Aorta and its ramifications, the Blood is of a bright scarlet colour, but, in its progress through the system, it assumes a darker hue, and when brought back by the Venæ Cavæ to the right side of the Heart, it is of a deep purple.

After passing through the Pulmonary Artery to the Lungs, it is exposed to the atmospheric air, inhaled in respiration, is unloaded of its Carbon, and returned by the Pulmonary Veins, purified and florid, to the left side of the Heart.

The Body, it appears, undergoes a frequent change of its particles; the old constituents being worn down and useless, new ones are continually needed for the repairs of the structure, and the due performance of its functions. From the Blood is formed the nourishment for the growth of parts, and for the reparation of their daily waste. It is the source, likewise, of the adhesive matter, poured out for the union of wounds; and it affords the pabulum of those granulations, which fill up the cavity of ulcers. By this fluid, also, is taken up the useless and decayed parts of the animal frame; for in the course of the Circulation, Carbon is perpetually absorbed, producing the dark colour of venous Blood. In the Lungs, as was stated, the blood is freed from this substance;* and the quantity which Experiment has proved to be

* Lavoisier, Priestley, and Davy have ascertained this fact by experiment; and more recently the accurate researches of Allen and Pepys have amply confirmed their statement.

emitted by expiration is truly astonishing.* Not less than eleven ounces of Carbon in the form of Gas, is thrown off, it appears, in twenty-four hours.†

Nor is this the only use of the blood connected with respiration. It performs, or at least is the principal agent in performing, the Calorific function. The Blood on its exposure in the Lungs to the action of the atmospheric air, is believed to imbibe latent Caloric, and this, in the course of the Circulation, gradually to evolve for the maintenance of the animal temperature.

The Blood, moreover, is requisite to the actions of the Nervous system, for if vascular communication be destroyed, the sensibility of a member is lost.‡

While circulating in its vessels, the Blood is, generally speaking, a homogeneous fluid ;§ but on

* Allen and Pepys in the Philosophical Transactions.---See also Davy's Researches.

† Dr. Priestley entertained the idea that a proportion of Azote is absorbed by venous Blood during respiration, and Sir H. Davy had adopted his opinion ; but from the experiments of Allen and Pepys, it appears that no perceptible quantity of this Gas is consumed in the Lungs.

‡ Haller, on tying the abdominal Aorta of a Dog, found a total paralysis of the lower extremities to succeed. This Experiment has also been frequently performed by Mr. A. Cooper with a similar result.

§ One exception is met with in those parts of the body which are supplied only with colourless Blood : and another occurs, where the Chyle

its removal from the body, a thin film forms on its surface, it concretes, separating afterwards into two parts, one, a thick red cake termed *Crassamentum*, the other, a pale yellowish fluid denominated *Serum*. The former usually floats in the latter; but instances have been recorded by De Haen, Hunter, and Hey, of the *Crassamentum*'s enveloping the *Serum*.

It has long been disputed whether or no heat be evolved during coagulation. Hunter, from his experiments on Turtles, inclines to the negative, but some modern chemists assert the evolution of Caloric. The question remains as yet undecided.

The period, at which coagulation commences, depends on circumstances, which will be afterwards considered, but the common time is from three to eight minutes after the Blood has been taken from the body. The completion of this process is effected generally in from one to three hours; but frequently a much longer time is required, the Blood being found but partially

is first received into the sanguineous system. It would seem probable also, that the Blood returned *from* the secretory organs differs from that conveyed *to* them by their Arteries. As far, however, as the subject has been examined, the supposition remains unverified.

coagulated at the end of twenty-four. And even when the separative change appears to take place in the usual time, Serum will often exude from the Crassamentum for several days, especially if the fluid be repeatedly poured off from the coagulum.* After being exposed to the air for some hours, the exterior of the Crassamentum imbibes Oxygen, and assumes a florid hue, this change resembling that produced in the Lungs by respiration. It is observed, however, that if the cake be not kept moist in the Serum, instead of becoming scarlet, the colour is that of dark brown.

After the Blood has remained two or three days in a temperature of 60°—70° putrefaction commences, and ammonia is evidently evolved.† And by the continuance of this process, the Blood is soon reduced to a black fetid mass.

The quantity of Blood circulating in the human structure has been variously computed by Philo-

* Boerhaave, however, and his commentator, evinced great want of observation in asserting that the Crassamentum is almost entirely dissolved into Serum. “Resolvitur in Serum ita, ut in id omnis fere cruor abeat.”—Aphor. 94. “Apparet, rubram partem sanguinis sensim quasi deliquescere et in serum abire.”—Com.

† It appears that this change occurs sooner in venous than arterial Blood. *Experiment.* Blood was taken in separate vessels from the Jugular Vein and the Carotid Artery of a Dog during the summer of 1813. On the second succeeding day, that of the former emitted a strongly putrid smell, while that of the latter was almost devoid of fœtor.

sophers. Keil has estimated it at 100 lbs.; others do not believe it to exceed 8 lbs. Haller computes it at 10; Young at 40; and in Cooper's Lectures, its proportion to the solids of the body is considered as 1 to 16 or 20. The wide difference in these estimates seems to depend on the want of such data as are requisite for accurate research. It is true, indeed, that the contents of the principal Arteries and Veins may be subjected to calculation, but of the Blood circulating in the Capillaries, no accurate estimate can be formed. When we reflect on the minuteness of these Capillaries, on the universality of their distribution, and on the large proportion which they constitute of muscle and other solids, we must conclude that 8 or 10 lbs of Blood is a quantity far from adequate to their supply.

From the difficulty of accounting for the changes which the Blood undergoes on its removal from its vessels, a celebrated Physiologist started, or rather revived, the opinion of its possessing life.*

* The idea of the Blood's vitality was entertained by the ancients.—In many passages of the writings of Hippocrates, the connection of the Blood with the soul or spirit is clearly intimated.

“ Ηγεομαι δε εμπροσθεν μηδεν ειναι μαλλον των εν τω σωματι ξυνβαλλομενωνες φρονησιν αν, η το αιμα. Τετο δε οταν εν τω

On this subject, I enter with reluctance, conscious that the Veil, which conceals from our view the principles and agency of the Nervous system, precludes any legitimate conclusions on the existence or operation of fluid vitality. Till that veil shall be rent, till the properties, diffusion, and laws of animal life shall be developed, Physiologists will ever hold opposite opinions on subjects of this nature. An inquiry into the principles of the Blood, however, obliges me to advert to the opinions of Mr. Hunter, and the reasons on which they are founded.

His ideas, he informs us, first arose from observing that the yoke of the egg resisted putrefaction, though exposed to a continued temperature of 103 degrees.

That the Egg has “the power of self preservation, or in other words, the simple principle of

καθεστηκοτι σχηματι μενη, μενει και η φρονησις, εξαλλασσοντος δε τσ αιματος, μεταπιπτει και το φρονημα.”—DE FLATIBUS.

The illustrious Harvey also held this doctrine, and expressed it in decided language:—

“Habet (seil. Sanguis) profecto in se animam primo ac principaliter, not vegetativam modo, sed sensitivam etiam et motivam: permeat quoquoersum, et ubique presens est; eodemque ablato, anima quoque ipsa statim tollitur: adeo ut sanguis ab anima nihil discrepare videatur; vel saltem substantia, cujus actus sit anima, estimari debeat.”—DE GENERATIONE.

life", will be readily admitted, but a question remains, Does this principle reside in the yoke, or the albumen, independent of the chick? For the chick is an organized animal, remaining enveloped in these substances till the stimulus of heat bring it to maturity.

The argument drawn from mortification's taking place in a member, when the circulation is destroyed, only proves that the Blood is necessary for animal life. So is aliment. If we remain for a certain time without food, death ensues: yet no one asserts, that the food we eat, possesses vitality.

Hunter also advances, in support of his theory, the union of wounds, observing, that extravasated Blood and adhesive matter, become vascular. Yet, though the extravasated clot, and adhesive matter effused for the reparation of injuries, be formed from the Blood, organization does not originate from their spontaneous action, but from the elongation of the surrounding vessels, which branch out into the deposit, as the roots of a tree extend themselves in the earth.

Mr. H. laid much stress on the idea, that if the Blood were not alive, "it would be in respect to

the body as an extraneous substance." If there exist in the animal structure certain canals, which are destined to convey certain fluids, these fluids, I conceive, cannot be considered as foreign to the vessels which contain them. Upon Hunter's principle, moreover, the Lymph is as truly endued with vitality, as the absorbents which convey it.

Mr. H. remarks also, that Blood taken from the arm in the most intense cold which the human body can bear, raises the Thermometer to the same height as Blood taken in the most sultry heat; and he conceives, that since living bodies alone have the power of resisting high and low degrees of heat, the Blood, from its possessing this property, must likewise possess vitality. It should be remembered, however, that life may maintain its temperature in the system at large, yet particular parts maintain *their* temperature only by contact. The various secretions have the same heat as the Glands or Muscles; the Urine, for instance, on flowing from the Urethra, raises the Thermometer to 98°. Yet surely this Physiologist would not have asserted the vitality of Urine.

Another difficulty attends Mr. Hunter's theory. If the Blood possess life, where is this property

first acquired? We know that the Blood is formed from the digestion of various inanimate substances. "Is the Chyle alive?" Mr. H. thinks it is. Admitting the supposition, we must ask, "Is the *Chyme* endued with vitality, or does our food, by commixture with the Salivary and Gastric juices, acquire its living principle?"

If in *vitality*, be implied an *independent power of motion*, we should expect, that the circulation of the Blood was spontaneous, and that its properties were of an active kind. But of such inherent faculty, we have no evidence. We observe a simply passive motion of this fluid: the Heart impels it, the Vessels convey it, the Glands act on it.

Perhaps the argument drawn from the coagulation of the Blood, is the most plausible of those adduced in favour of Hunter's opinion. If Blood, before concretion commences, be exposed to a low degree of temperature, it freezes; if it then be placed in a higher temperature, it thaws; and in from four to six minutes, it coagulates. If a piece of muscle, taken from an animal killed by violence, be frozen and afterwards thawed, it contracts. In both cases, it is evident, that freezing suspends,

but does not prevent, the natural action. Hence there appears a marked resemblance, between the death of a muscle and the coagulation of the Blood.*

Hunter pursued the analogy farther. In persons killed by Lightning, by mental emotions, blows on the Stomach, &c. he remarks, that the muscles do not contract, neither does the Blood coagulate. And, though he allows the coincidence not to be invariable,† yet, as a general observation, it is undoubtedly correct.

One simple analogical fact, however, it will be generally admitted, affords not sufficient grounds for the establishment of such a Theory ; especially when it appears, that the principal arguments on which it rests, would prove too much, leading to the conclusion that *fluid vitality* is not confined to the Blood.

* May not this analogy depend on the same fact, and be explained on the same principle, viz. the *quantity of Fibrine*, contained both in Muscle and Blood ? We are well assured that the coagulation of the Blood is effected by its Fibrine.

† An exception recently occurred to me on inspecting the body of a man who died suddenly. The muscles were quite rigid, but the Blood was fluid, nor did it, two hours after being taken from the Thorax, exhibit any other traces of coagulation, than a slight disposition to be grumous.

Such are the objections which present themselves on the review of Hunter's Ideas of fluid vitality; yet I am by no means *confident* that these Ideas are erroneous. I am aware that every Physiological opinion is liable to objections, and that it is much more easy to overturn a Theory than to substitute a better. Mr. Hunter was a man, who rarely advanced a doctrine without thought and research; and hence, in canvassing the merit of his opinions, we should have a reflection not less deep and observant, and institute an examination not less patient and minute, than those which formed the basis of these opinions.

CHAP. II.

CHEMICAL QUALITIES OF THE BLOOD.*

THE *specific gravity* of recent Blood has been stated by Haller at 1527,† Water being 1000 ; by Boyle‡ at 1041 ; by Jurin,§ at 1054 ; by Blumenbach,¶ at 1050 ; by Henry,|| at 1053 to 1126 ; and by Dr. Whiting, whose Experiments** are deserving of particular regard, at 1055 to 1061. Some variety, no doubt, arises from the state of the system at the time of the Blood's being taken, but much more from the degree of accuracy, with which the examination is conducted. My own observations, though marked by considerable diversity, lead to the conclusion, that the mean gravity may be estimated at 1050.

The *solid contents of the Blood*, also, have been differently stated, the causes of disparity being

* After the accurate Analyses which have been made by Berzelius, Bostock, Marcet, and other eminent Chemists, I should not have inserted any remarks on the subject, if Mr. Cooper's proposal had not required a Chapter on the "Chemical Qualities of the Blood."

† Physiol. ‡ Hist. of the Humane Blood. § Phil. Transact.

¶ Inst. Physiol. || Elem. Chemistry. ** Disputatio Med. de Sanguine Ægrorum.

probably the same as in the estimate of the specific gravity. Hoffmann found 4 ounces of Blood to yield on evaporation 1 Ounce.* Vieussens obtained 3 Ounces 7 drams of grey ashes from 50lbs. of exsiccated and calcined Blood.† Berzelius and Marcet state the fluid parts to be in the proportion of 900 to 1000, but Bostock rates it at $\frac{880}{1000}$.‡ Whiting ascertained the ratio in healthy Blood, to be $\frac{770}{1000}$ in one case, and $\frac{794}{1000}$ in another. With these observations my own nearly accord. On evaporating Blood to dryness, I have found the solid contents to be in the proportion of about 1 to 4; or the watery parts to be as 750—850 to 1000.

The temperature of the circulating Blood, is 98°—102°; and its heat is said to be the same in Arteries and Veins in proportion to their vicinity to the heart.

Exposed to distillation, the Blood affords Hydrogen, Carbonic Acid Gas, and Azote, Ammonia, Prussic Acid, Inflammable Oil, Phosphate of Lime, (Phosphate of Iron?) and Salts of Soda.

* Philos. Corporis Humani.

† Phil. Transact.

‡ Med. Chir. Trans. See the Tables at Page 1 of the Appendix.

In stating the general properties of the Blood, its spontaneous separation into Serum and Crassamentum was noticed. The *proportion which the former bears to the latter*, varies according to the state of the system, but its medium in health is usually $\frac{10}{13}$ to $\frac{10}{14}$,* or as 1 part Serum† to 1.3—1.4 Crassamentum. Langrish,‡ on bleeding three young men, wholly free from disease, found the fluid part to be considerably more than $\frac{1}{3}$ though it did not reach $\frac{1}{2}$. The proportion of the *whole* Serum, i. e. of the fluid portion of the coagulum artificially separated, as well as that naturally exuded, is stated by Jurin at $\frac{15}{17}$ of the entire Blood. Diet and Exercise, however, produce a considerable alteration in the relative quantites of Serum and Crassamentum.

SECT. I. SERUM.

The specific gravity of the Serum has usually been stated at 1020 to 1030. Boyle and some others have estimated it much higher, but their experiments were objectionable. The medium

* This estimate, I scarcely need observe, is formed from weight.

† The *separable* part of Serum; for the Insula contains, after the completest coagulation, a considerable portion of fluid.

‡ Theory and Practice of Physic

is considered by Bostock, 1023, and by Marcet, 1029. Jurin states it at 1028—1030. My own examinations of Serum convince me that its specific gravity is subject to great variety. I have found it as low as 1004 and as high as 1080:* but the medium may be fairly rated at 1020—1040. Serum contains Albumen, Water, the Carbonate Sulphate and Muriate of Soda, Muriate of Potass, the Phosphates of Soda and of Lime. Bostock found the Muriate of Soda to be in the proportion of 1 to 300.

The *solid contents, on evaporation*, are stated by Bostock at 12 per Cent. The average of my own observations is somewhat lower.

The Sulphuric and other mineral Acids, readily coagulate Serum, separating its albumen in the form of a flaky precipitate. Without the action of chemical agents, Serum remains fluid in the temperature of the atmosphere, but coagulates on exposure to a heat of 150°—160°. A semi-transparent clot is formed, of a yellowish hue, with a surface freckled and cupped. The pellicle on the top has an oily appearance, beneath this is a blu-

* These observations, it is proper to remark, were not made on healthy Blood.

ish substance, and at the bottom an ochrey mass. The coagulum of Serum is principally Albumen. It consists of Carbon, Azote and Oxygen, containing also a large proportion of Sulphur. This, according to Proust, is combined with Ammonia in a state of Hydro-sulphuret.

From coagulated Serum may be pressed a small quantity of fluid, which has obtained the name of *Serosity*. In this, the salts are chiefly found. It is water, holding in solution about $\frac{1}{50}$ of albuminous matter,* with a large proportion of alkaline salts.† The solid contents are stated by Bostock at $\frac{1}{46}$ to $\frac{1}{70}$.

From the analysis of Serum compared with that of the secretions, it is apparent that this fluid is their principle constituent.

SECT. 2.—CRASSAMENTUM.

THE Crassamentum is separated by ablution into two parts, viz. the *Fibrine* and the *Red Particles or Cruor*.

1. *Fibrine*.—If recent Blood be stirred for a few minutes, an adhesion of the coagulating

* It was generally understood that the Serosity was *gelatinous*, till the experiments of Brande and Bostock proved that no Jelly exists in Serum.

† Brande in Phil. Tran.

Lymph, to the rough surface of the stick takes place ; and this flaky substance, losing, on repeated washing, its red Particles, shews distinctly and beautifully, the Fibrine which forms its basis.* It has the appearance of white strings laid in striæ, and resembles, in form and distribution, the planes of muscle which encircle the Bladder. Fibrine consists of Azote, Carbon, and Oxygen ; the first of which is found in greater proportion than in any other animal substance. There exist also, according to Hatchett,† some traces of Albumen. The Fibrine is with difficulty soluble either in Sulphuric Acid, Alcohol, or solutions of Ammonia, and is but little affected by other chemical agents. The proportion which it bears to the entire Blood, is stated by Berzelius at $\cdot 75$ to 1000 ; but Whiting, from his Experiments, concludes it to be from 1 to 2 in 1000. The Fibrine is well known to form the basis of Muscle ; and so nearly does it approach to organised matter that the Galvanic Aura increases its contrac-

* Malpighi first described this interesting appearance :—" Contexturam namque fibrosam et quasi nerveis fibris compaginatam rete videbis, in cujus exiguis excitatis spatiis et sinibus, velut cellulis, rubicundus stagnat ichor."

† Phil. Trans.

tion. From it are formed the various parts of the animal Frame, as well as the preternatural structures resulting from disease; hence its names of Plastic Lymph and Coagulable Lymph.

2. *Red Particles*.—The *specific gravity* of the Blood-globules, is to water, nearly as 1·2 to 1. Jurin states it at 1277, water being 1000; and in another, though incorrect mode, he estimated it at 1126.*

From Berzelius' analysis, it appears, that the ratio which the red Particles bear to the other constituents of the Crassamentum is as 64 to 36.

Their *Diameter* has been variously estimated; $\frac{1}{1500}$ ^{ths} of an inch, $\frac{1}{3300}$ ^{ths}, $\frac{1}{4000}$ ^{ths}, and $\frac{1}{4500}$ ^{ths}.† The most recent examination is that of Sir E. Home, and Mr. Bauer. It states the diameter of a Globule, devoid of its colouring matter, to be $\frac{1}{2000}$ ^{ths}.§

The *Form* of these Particles has been a subject of dispute with Philosophers. Lewenhoeck, Hewson, and Cavallo have been warmly engaged in the controversy; and though each determines

* Dr. Jurin, though generally accurate in his researches, seems to estimate the Red Particles and Fibrine collectively.

† See the Writings of Haller, Blumenbach, Wollaston, &c.

§ Phil. Trans, 1818.

their figure from microscopical observations, each gives a different account of what he beheld.—Lewenhoeck represents them as circular at rest, and elliptic in motion : influenced by the excess of a laudable spirit of discovery, and allowing fancy to take the lead of careful observation, he states, also, that each principal globule is composed of six minor and separable globules. Haller and Senac compare the red Particles to Lentils ; yet the former, in one part of his works, asserts their spherical form, and doubts their change on motion. According to Hewson and Wells, they are flat, with a vesicle in the centre, containing a solid substance ; according to Cavallo, they consist of double spheres : by Blumenbach they are considered as simply globular, and in motion oval : Hunter believed them globular, and similar in size in the same animal.

Unaccustomed as the eye must be to comparative calculations on objects so minute, we cannot be surprised at the disparity of these opinions, particularly when we reflect on the deceptive representations produced by the refraction of the rays of light. The uncertainty, however, respecting the figure of the red Particles, is the less to be

regretted, since an accurate knowledge of the subject would not probably lead to any physiological or pathological deductions; unless it should appear, that they undergo a particular change of figure in passing through the vessels, or that the “Error loci” of Boerhaave is the real cause of Inflammation, or of Fever.

The Cruor is readily dissolved in water and lemon juice, but with much less facility in vinegar. Solutions of most neutral salts, as the Nitrate of Potass, the Muriate of Ammonia, the Sulphate of Magnesia, and the Salts of Soda, the Mineral Acids, also, reduced to a low degree, have no other effect on the red Particles, than a change of colour; the Nitrate of Potass giving them a scarlet hue, and the Mineral Acids producing a dusky brown. Until of late years, it was generally believed, that the Cruor consisted principally of iron. The experiments and observations of Fourcroy, Vanquelin, Parmentier, and Berzelius, induced the opinion that this Mineral existed either in the form of Oxyd, Subphosphate, or at least as the elements of these substances. But Wells,* endeavoured to overthrow this opinion:

* Phil. Trans. 1797.

later experiments render the common idea still more problematical ; and many modern chemists find but slight traces of iron to exist in the Blood.* The red Particles, however, are slightly attracted by the magnet.

Berzelius and Brande have ascertained that the colour of the Blood does not depend upon iron : they conceive that it is produced by a peculiar animal dye.

That the changes we observe in the colour of the Blood, are produced by the Oxygen of the Atmosphere, is evident from the effects of Respiration, as well as from the daily observation, that when Blood is drawn from the body, its surface becomes scarlet, while the interior parts remain of a deep carbonized hue, till their position be altered, and the air freely admitted. If Blood be exposed to Oxygen, the bulk of the Gas is diminished in proportion to the scarlet colour which is induced : Carbonic Acid Gas, on the contrary, quickly deepens the hue of arterial Blood ; and hence we should conclude that the absorption of Carbon is the cause of the Modena. But Priestley found that Arterial, assumed the cast of Venous

* Hatchett and Brande in Phil. Trans.

Blood, though placed in vacuo. Hydrogen removes the purple hue of the Blood, but Azote and Nitrous Oxyd render it dark.

The colour of the Blood in the circulation, is heightened by exercise and proper diet, and diminished by a sedentary life, and a bad, or scanty supply of food. Repeated bleedings render it much paler; and hence it appears that the Globules are not of comparatively easy reparation.

Haller has remarked, that the Blood of young and debilitated animals is of a pale yellowish cast. I have never observed this variety either in whelps and other young animals, or in the feeble and languid. Between the young and the adult, there exists scarcely any difference; and in the Blood of the debilitated and the strong, no other disparity than a shade of red.

Medicines containing Iron are known to increase the hue of the Blood; and this effect has been attributed to the absorption of the mineral into the circulating fluids.—The cause, however, more probably originates in the tonic stimulus of the medicine on the Alimentary Canal and the consequent activity of the vascular System.

SECT. 3.—HALITUS.

The steam which arises on the subtraction of Blood is found to consist of Hydrogen and Carbon, and differs little from common water. It is nidorous to the smell.

CHAP. III.

PECULIARITIES OF THE BLOOD IN DIFFERENT CLASSES OF ANIMATED BEINGS.

WHEN it is considered, that in most animals of the higher orders a marked similarity exists,—that, although some peculiar structures are provided for the particular situation or habits of these creatures, yet all have the systems of circulation, low-motion and sensation, all have organs of Digestion and of Secretion common in their nature and similar in their action,—we cannot expect to find any considerable variety in the character of the Blood. This opinion is supported by a remark of Hunter's. He has stated that no material difference exists in the Blood of different animals, except in the proportion of red Particles. “Transfusion of the Blood of one animal,” as he justly observes, “into the vessels of another, proves to a certain degree the uniform nature of the Blood.”* Nor does it appear requisite, that the Animals on which this Experi-

* Hunter on the Blood.

ment is made, be of the same tribe.* The Blood of a carnivorous creature will circulate in the vessels of the graminivorous, and the Blood of the graminivorous will circulate in those of the omnivorous.

To form a complete comparative statement of this fluid, as it exists in the different orders of the animal kindom, if it were an inquiry either practicable or desirable, would require ample opportunities, much leisure, and the nicest observations. I have examined, however, the Blood of those Animals which I was able to procure; and although I regret the time spent in this research, yet, since even negative knowledge is not always devoid of utility, I proceed to state the results of my inquiries, connecting them, at the same time with the Experiments and remarks of others.

I. The quantity of Blood in proportion to the bulk of the animal, is believed to vary in different tribes. Dr. Moulin† examined the weight of

* In an inaugural Thesis published by Dr. Leacock, "De Hemorrhagia," several Experiments on Transfusion are related. At the conclusion he remarks "—— docet sanguinem ovis, animalis scil. herbivori, posse sustentare canem, animal contra carnivorum." See also the early volumes of the Phil. Transactions.

† Phil. Trans.

slaughtered animals before and after death. He found that a Sheep, whose weight was

118 lbs. lost $5\frac{1}{4}$ lbs. of Blood, or 1 in 22·4 of the whole weight.

A Lamb 1 in 20·3

A Duck 1 in 28·6

A Rabbit..... 1 in 29·2

From these statements it appears that the Lamb has a greater proportion of Blood than the Sheep, the Sheep than the Duck, and the Duck than the Rabbit. Conclusions from such premises, however, are by no means satisfactory; since the estimate is formed from the quantity lost by hemorrhage, and it is well known that in such circumstances, the particular state of the Nervous system, as retarding or accelerating death, will materially affect the flow of Blood, and consequently render the calculation inaccurate. As a general observation, however, I believe that *Birds, Fishes, and the weaker animals, have a less relative quantity of Blood than the larger and more muscular*; and that in the highest orders of the creation, in man especially, the proportion is by far the greatest.

II. The point on which I expected most diversity was in the relative quantities of Serum and Crassamentum.

In <i>Dogs</i> I found the Serum in proportion to	}	10 to 20 or 25*
the Crassamentum, as		
In <i>Oxen</i> the average appeared to be as		10 to 16
In <i>Horses</i> ,		10 to 13
In <i>Sheep</i> , the medium of some parcels has been		10 to 21
while in others, it was but as		10 to 8!
In <i>Swine</i> ,		10 to 18
In <i>Fowls</i> ,		10 to 16

Although my Experiments are far from evincing a disparity uniform in its reference to the classes of animals, yet it appears probable, that a more complete examination would prove the *Crassamentum* to bear a proportion to the strength and ferocity of the Animal; since I never found the Serum in such quantity as in the timid sheep, nor the Crassamentum so abundant as in the predatory dog.†

III. The *periods of Coagulation* have also been compared with those of human Blood.

In the Blood of the Horse concretion‡ occurred in from 5 to 13 minutes.

In that of the Ox from 2 to 10 minutes.

In that of the Sheep, Hog, and Rabbit $\frac{1}{2}$ to $1\frac{1}{2}$

In the Blood of the Lamb $\frac{1}{2}$ to 1

In that of the Dog..... $\frac{1}{2}$ to 3

* My estimates of the relative quantities of Serum and Crassamentum have always been formed from weight.

† In one case it was 33 to 10 of Serum.

‡ By *concretion* I mean the commencement of coagulation.

In that of the Duck 1 to 2.....
 In that of Fowls $\frac{1}{2}$ to $1\frac{1}{2}$
 Haller observed, that the Blood of a Mouse coagulates in a
 moment.

From these observations a general inference may be drawn, that *Coagulation commences sooner in small and weak animals, than in the large and strong.*

IV. *Temperature.*—It has been generally understood, that the heat of the Blood does not vary in Animals of the higher orders. Braun,* however, has stated that the Blood of a Calf raises the Thermometer to 104° , and that of Birds to 107° — 110° . Doubting the fact, I examined the Blood of the Ox, Horse, Sheep, and Duck, in reference to this subject. In the Horse, the Temperature of the flowing Blood was 97° ; but in the Ox, 100° — 101° ; in the Sheep, 102° — 103° ; and in the Duck 107° . It is not, therefore, without reason, that Braun has asserted *the Blood of Birds, and many animals of the class Mammalia, to maintain a degree of heat higher than that of man.*

V. In the *Chemical Qualities* of the Blood of Animals, no considerable variety has been found to exist. Berzelius states the Blood of the Ox,

* Diss. Physica Experimentalis in Comms. Acad. Petropolitanae.

to differ in no respect from that of the human subject, except in containing a smaller quantity of Saline matter, and a larger proportion of Azote. The latter, however, is a circumstance curious and unexpected, when we consider that man lives in a great measure on animal food, while the Bullock's sustenance is wholly vegetable.

The *specific gravity of the Blood* of Animals, and the *solid contents* on evaporation, have been found to bear a general resemblance to those of the human subject, as far as my Experiments have been prosecuted.*

The *Serum* of animals presents no marked diversity, either in its appearance, its taste, its qualities or its coagulation.† In fat animals I have often noticed oily, or rather adipose, matter floating on the surface of the Blood: on cooling, this substance has become a white concrete mass.

On the venous Blood of Whelps, I have more than once remarked a cream-like crust. It was thickest at the edge of the coagulum, though milky streaks pervaded the Serum.

VI. The *red Globules* exist principally in the

* In the appendix some particulars are stated.

† See Appendix.

more perfect animals;—in the Mammalia and Birds, partly in Fishes, but not generally in Reptiles, Insects and Worms. In some creatures, coloured Blood is found in the vessels near the Heart, while the rest of the body is supplied only with a serous fluid. We observe some Fishes to have red Blood afforded to particular parts, while the contents of the vessels in the principal structures are pale. In the Skate, for instance, the Fins have ramifications of scarlet vessels, yet the rest of the body is white. In Frogs, the Blood brought from the Intestines is represented by Haller to be of a pale yellow: he also observed, in the same reptile, two columns yellow and purple, resisting each other in the same vein.* The blood-globules, in the Amphibia, and those creatures whose circulation is slow, are much larger than in man;† while in some animals, they are said to be considerably smaller. Of those creatures, which want the red Particles, most have *white* Globules, but in the lowest orders, even

* Second Dissert. on the Blood.

† Baker in speaking of the water-sow, states that the Globules “appear about ten times as large as those of the human Blood, and their progressive motion is very slow and languid, whereby they become more distinguishable than the globules are in the Blood of animals, whose circulation is swifter.” Employment for the Microscope.

these cannot be discerned by the Microscope. In such minute animals, it is impossible to ascertain the quantities of the other constituents: reasoning, however, from analogy, we should conclude that the proportion of Fibrine is small.

VII.—The Blood of some creatures is found, while circulating, to contain air-bubbles. In the Land and Sea Tortoises, in some Fish, in the Hedge-hog and the Viper, this appearance has been asserted by respectable Writers.*

* Morgagni de Sed. et Caus. Morb. Epist. V. 22.

CHAP. IV.

COAGULATION.

IN stating the general properties of the Blood, the common Phenomena of Coagulation were detailed. It is now requisite to notice the effect of certain circumstances in retarding or accelerating this process, as well as the operation of some chemical agents.

1. Solutions of the Sulphate of Soda, Nitrate of Potass, and of most other Neutral Salts, it is well known prevent the complete coagulation of the Blood. The diluted mineral acids also, and Bile* have a similar effect. Water added to Blood has no influence on the period of coagulation, unless its relative quantity be considerable. A solution of Opium retards, though it does not preclude, this process. It occurs also, when the Blood is mixed with the poison of the Viper.†

* If Blood be taken from the Hepatic vessels of an Ox, recently killed, it cakes, but never separates into distinct Serum and Crassamentum: this probably arising from the Blood's having partially assumed that change, which constitutes the biliary secretion.

† Mead's Medical Works, Fontana on Poisons.

The Bitters, Tobacco, and various Narcotics, impede the separation of the Blood, or render it less perfect.* A solution of Ammonia almost wholly prevents coagulation. Most Gaseous fluids accelerate it.

2. The *influence of Medicines*, internally administered, on the Blood's disposition to coagulate, is not, however, apparent. If Venesection be performed when the system is saturated with Mercury, the Blood I have found to assume its changes in the usual time. To ascertain the effect of stupefaction from opium in reference to this subject,

EXPERIMENT I.

A small cupful of Blood was taken from a Whelp, to which had been given two hours before, ten grains of Extract of Opium. Four days afterwards, I remarked, that the *coagulation was but partial*. This Experiment, however, was incomplete, since from accidental circumstances, the period at which the process commenced, was

* The writer of the article "Blood" in Rees' Cyclopædia, states, that he has frequently received Blood into strong decoctions of Belladonna, Tobacco, &c. and that he usually found a flacculent coagulum to form at the expiration of eight minutes. But it does not appear, that in any of his Experiments, the natural process was complete.

not properly noted. The Blood, moreover, of very young animals, rarely undergoes a complete separation.

EXP. II.

To a Dog was given half a dram of Extract of Opium ; and in two hours, the animal appearing to be completely under the influence of the Narcotic, a wound was made in the neck, and a Carotid artery divided. The Blood had a natural aspect, and *begun to coagulate in two minutes*. Weighed 35 hours after, the proportions were found to be — Serum 570, Crassamentum 1176, or as 10 to 20·6.

I conclude, therefore, that *intoxication from Opium does not affect the coagulation of the Blood*.

3. *The coagulation of the Blood is induced readily in proportion to its paucity*. If it trickle from a wound, or flow over an extensive surface, concretion, as daily observation evinces, almost instantly ensues ; but if, on the contrary, it issue in a full stream, and be received in a proper vessel, several minutes elapse before this process commences. Although, however, coagulation *begins* the soonest in proportion to the paucity of the Blood, the *complete separation* of the Serum and the Crassamen-

tum is in the *inverse ratio*; or at least, does not take place when the quantity is very small, or the coagulum thin. It is perhaps deserving of remark, that in some cases where the Blood, after death, is found fluid or grumous in every other part, it is coagulated in the heart;* the peculiarity originating probably in the larger quantity contained in this viscus.

4. *Agitation* is another agent in *retarding coagulation*.† The stirring of Blood as it flows from an animal, is frequently practised for culinary purposes, to prevent its caking. In this case, a portion of Crassamentum adheres to the stick, while the rest forms, with the Serum, a grumous mass.

5. The *influence of high and low degrees of temperature* on the Blood's coagulation has often been erroneously stated. To ascertain the precise effect of these agents, the following Experiments were made:

EXP. III.

On bleeding a patient, one cup, containing about an ounce of Blood, was placed in a temperature of

* Morgagni relates several instances in his Epistles.

† It was remarked by Hippocrates:—*Ην δε τις αυτο (i. e. αιμα) τινασσει, & πηγνυται.* De Carnibus.

120°, and another of similar capacity, in a temperature of 40°. The first, with a sizzly surface, was strongly concreted in two minutes; the second, without any size, began to coagulate in 2 min. 10 sec. The time requisite for coagulation in the temperature of the atmosphere (55°) was found to be 4—5 minutes.

EXP. IV.

On taking Blood from the arm, the first cup was placed in water heated to 110°, and the second on the table, temperature about 60°. The first, buffy, coagulated in 2½ minutes, the second without buff, caked in two minutes.

EXP. V.

A similar Experiment, no trace of the buff:—

1. Temperature 45° coagulated in 2½ minutes.
2. 70° 4

These observations lead to the conclusion, that both high and low degrees of Temperature hasten the Blood's concretion; and if it be remembered, that this process is greatly retarded by the presence of the sizzly tunic, IV. supports the opinion as strongly as V. Among other Experiments which I made on the subject, one* seemed to contradict

* In bleeding a lad labouring under an enlargement of the Axillary Glands, three cups were taken:—

the inference I have drawn; but, as the rest bore an opposite character, I conclude this exception to have arisen from some fortuitous cause. The general result has been, that of degrees of heat from 40° to 120° , *the Blood concretes soonest in a temperature of 100° — 120° ; next in one of 40° — 50° ; and last, and with a greater disparity, in that of 60° to 90° .* It is deserving of remark, however, that the *Serum is most readily and copiously effused in the higher temperatures*, and this I believe in regular gradation. Hewson and Hey agree in stating, that the nearer the heat is to that of the human body, the more speedy and perfect the separation; but experiment will evince a temperature of 120° , to cause a more speedy and perfect coagulation, than one of 98° .

Mr. Hey has observed, that a heat of from 39° to 46° , frequently prevents altogether the separation of Serum. I have often placed Blood in a room

1. Placed in a cold solution, 42° , coagulation commenced in 6 mins.
2. Placed on a table,..... 45° , 4
3. Placed in water heated to 98° , 3

The disparity in the periods of coagulation in numbers 1 and 2 may, perhaps, be attributed to the state of the system. Aware that coagulation is accelerated by that debility which bleeding induces, I generally filled the cups, in these Experiments, as nearly at the same time as possible. In this case, however, I recollect the interval to have been longer than usual.

where the thermometer stood at 45° to 48° and 50° , but never, as yet, have remarked this phenomenon.

After the preceding remarks, it is unnecessary to confute the notion that the Blood coagulates at a particular temperature, as stated by some respectable authors.

6. A curious observation has been made by Mr. Hey, that if the flowing Blood be received into a cup containing a proportion of water, this fluid will be in a great measure absorbed by the Crassamentum during coagulation, the insula, in such circumstances, considerably exceeding that of the undiluted Blood.* Though a small quantity of water does not materially affect the coagulating process, yet a proportion of 1 to 2 will generally I believe produce the effect he has remarked.

7. Connected with the subject of coagulation, a question, not devoid of interest, may be started for discussion and inquiry. How far is it possible

* In one of Mr. Hey's Experiments the result was as follows :—

	<i>Serum.</i>	<i>Crass.</i>
1 Cup, containing an ounce of cold water.....	30.6	to 100
2 tepid water	45.0	.. 100
3 undiluted, but the vessel placed in a cold solution	40.8	.. 100
4 the tempera- }	62.9	.. 100
ture of the human body		

Appendix to Hey's Observations on the Blood.

for the fluidity of the Blood to be regained after concretion has occurred in its vessels? Coagulation to a great extent, it is obvious, must prove speedily fatal, but that it may take place in a minor degree without such an event, is rendered probable, by some observations of Hunter's and Haller's. The former states, from one of his correspondents, that the Blood of a Bat in its torpid state, is found in a certain degree coagulated, but that fluidity is quickly restored by heat and motion. From Haller's Experiments on Frogs, it appears, that a temporary concretion may be occasionally remarked, even when the vital actions are undiminished.

8. It has been stated, that the disposition to coagulate is greater in Venous than in Arterial Blood;* and this also in proportion to the vicinity of the vessel to the heart: nay, some have asserted, that arterial Blood never coagulates. The folly of the latter opinion, the operation of Arteriotomy will at any time exhibit;—in regard

* *Alio modo etiam differe a venoso sanguine arteriosum, ex eo colligimus, quod vix serum a sese seperare soleat, diu licet, asservatus; perfectiorem, intimioremque omnium miscelam hinc demonstrans,—De Haen, Ratio Medendi.*

to the former, the following observations were made:—

EXP. VI.

The Temporal Artery of a man labouring under an apoplectic affection, was punctured; and on the hemorrhage's ceasing after the effusion of about 4oz. of Blood, the cephalic vein was opened. In the first cup, containing Arterial Blood, coagulation commenced in *seven* minutes, while in the second, containing Venous, concretion took place in *three*. The stream, in both cases, was small.

EXP. VII.

The external Jugular and Carotid Artery of a lean Dog were punctured in immediate succession, and the Blood received in different vessels. Each portion concreted in from 30 to 40 seconds; but it was remarked that the Arterial Blood assumed the change 10 seconds later than the Venous.

These observations then, as far as their influence extends, favour the doctrine of *coagulation's occurring soonest in Venous Blood*.

8. On opening the bodies of Dogs and Rabbits immediately after death, I have not observed any marked difference, either in the period of coagu-

lation, or in the subsequent appearance, of Blood taken from the Pulmonary Veins and Venæ Cavæ, as contrasted with that of the External Jugular and other vessels. I have thought, however, that which is drawn from the Inferior Cava and Vena Portæ to want the uniform character of other Blood, and to be somewhat turbid or muddy.

Haller observed, that the Blood of the Spleen has a remarkable indisposition to coagulate,* and manifests rather the character of foetal than of perfect Blood.

10. A subject, which has particularly engaged my attention, is the *comparative periods of coagulation, as influenced by the strength or weakness of the vascular action*. This is an inquiry of considerable practical importance, as well as physiological interest; and hence I am solicitous that the statements should be fair and explicit. Like many other physical subjects, it has afforded matter of controversy: experiments have been recorded by Hewson, on the one side, and experiments of contrary result, advanced by Mr. Hey. Is it not surprising, that in philosophic researches, men of

* "Is sanguis vix unquam coagulatus est."—Primæ Lineæ.

eminence and veracity, should not only maintain opposite opinions, but support them by an appeal to similar experiments, similar facts, and similar observations? Many of the inquiries, indeed, connected with our profession, are of such intricacy, or such delicacy, as to render experiments indecisive in their results, or capable of being wrested by the bias of the experimenter to the support of a preconceived theory. But on this subject it is astonishing that a difference of opinion should exist, the experiments are so readily made, the results so distinctly evident, and so generally, if not universally, accordant. To ascertain the point in dispute, I instituted repeated experiments on the Blood of Oxen, Sheep, Horses, Dogs, and Swine, carefully noting the periods of coagulation as connected with the state of the vital powers. Aware of what I have before remarked on coagulation's commencing speedily in proportion to the paucity of the Blood, I took care that no disparity in the size of the stream should invalidate the result of my inquiries—each vessel receiving its contents from a full uninterrupted flow. To avoid unnecessary details, the following experiments only are stated.

EXP. VIII.

A Dog was bled to death, by dividing the vessels of the Neck. The Blood received in a glass, on the first gush from the wound, began to coagulate* in 1 *min.* 10 *secs.* A second portion, taken about 2 minutes afterwards, began to concrete in 40 *seconds.* A third quantity received immediately before the death of the animal, became *instantly* caked.

EXP. IX.

The throat of a Whelp, six weeks old, was divided, and the Blood received in a small vessel. Coagulation commenced in somewhat *less than half a minute.*

The first received Blood from a wounded Dog, usually begins to concrete in 1—3 *minutes.*

EXP. X.

Blood was received in three small cups, from the neck of a slaughtered Sheep. The first was

* It should be remarked, that in these experiments, coagulation was noted as commencing, when a clot could be perceived, and not where the Blood had assumed a concrete form. Hence, this process may appear to have taken place sooner in the experiments here detailed, than in those of others. Should any one, however, be disposed to re-examine the subject, I would recommend the last mode in preference to that I adopted; for the results, though the same in both, are most easily seen by allowing the Blood wholly to lose its fluidity, before the period of coagulation is noted.

filled immediately on the knife's dividing the vessels ; the second about $1\frac{1}{2}$ minute afterwards, and the third, a few seconds before death. The time which elapsed between the filling of the cups, and the commencement of coagulation, was, in the first, $1\frac{1}{2}$ minute, in the second, 1 minute, and in the third, *half a minute*.

EXP. XI.

Under similar circumstances, two small cups were filled ; the first, on the incision's being made, and the second, when the animal was greatly reduced.

	m.	s.
Coagulation commenced in No. 1,.....	1...	10
..... No. 2,.....	0...	50

EXP. XII.

The Blood of a slaughtered Ox was received in three cups ; No. 1, being filled on the first flow ; No. 2, about three minutes afterwards, and No. 3, a short time before the death of the animal.

	m.	s.
Coagulation commenced in No. 1,.....	3...	40
..... No. 2,.....	6...	45
..... No. 3,.....	0...	55

EXP. XIII.

A similar Experiment.

	<i>m.</i>	<i>s.</i>
No. 1	2	29
2	8	30
3	0	30

XV.

A large Ox.

	<i>m.</i>	<i>s.</i>
No. 1	2	30
2	1	35
3	1	10

XIV.

A struggling Ox.

	<i>m.</i>	<i>s.</i>
No. 1	2	50
2	1	10
3	2	15*

XVI.

A slaughtered Hog.

	<i>m.</i>	<i>s.</i>
No. 1	1	30
2	0	50
3	0	20

EXP. XVII.

Blood was received from a stuck Horse, at four periods, about $1\frac{1}{2}$ minute intervening between the filling of each cup:—

	<i>m.</i>	<i>s.</i>		<i>m.</i>	<i>s.</i>
No. 1	11	10	No. 3	9	55
2	10	5	4	3	20

Of my remaining Experiments on Animals, several, from accidental circumstances, were rendered inaccurate, but the rest so much resemble those which I have detailed, as to require no particular notice. From these statements, then, it appears, that in the Dog, Sheep, Horse, and Hog, the *Blood coagulates slowly in regular proportion to the Tonic state, or that condition of the system in which the vital powers are strongest*: the Blood

* In but one other Ox, and this a remarkably unruly animal, did I find coagulation commence late in the last cup.

received immediately before the death of the animal, first assumes this change ; next, that which is taken at the middle period ; and lastly, that which is received on the first effusion of the wound. In Oxen, however, it was frequently found (XII and XIII) that coagulation took place most slowly at the *middle* period. At first I was at loss to account for a circumstance so much at variance with almost every other observation. On reflection, however, I concluded, that the mode of slaughtering Oxen was the cause of the disparity ; the animal being first stunned with repeated blows on the head, and afterwards bled to death by the division of the Jugular vessels. The Ox, on falling, I conceive to be in a state resembling apoplexy, with the vital powers languid ; but after the loss of a considerable quantity of Blood, the nervous system to be relieved of its burden, and the constitution to regain somewhat of its wonted vigour. Here, then, the powers of life are *strongest in the middle period ; next so in the first, or comatose ; and weakest, after great evacuations, or on the eve of death.* I have noticed, that when a comatose state is not induced, either from the inexperience of the butcher in striking the Ox, or from

the Bullock's being more than ordinarily tenacious of life, the coagulation commonly takes place in the same order as that which occurs in other animals. The separative change was twice observed to commence *slowly* in the last received portion of Blood, (Ex. XIV.) but this exception being only found in the Ox, may be attributed to the deranged state of the nervous system.

The general inference drawn from my Experiments on this subject, is corroborated not only by repeated observation, occurring when the object of my research was unconnected with the period of coagulation, but by the testimony and casual remarks of others. It is unnecessary to quote the able work of Mr. Hewson, since he laboured to substantiate the doctrine which my Experiments have led me to support. Whoever studies the History of Experiments on the Blood, will generally find, that the Authors either intentionally or heedlessly, state the fact of coagulation's occurring most readily when the system is most reduced.*

* Dr. Jones, in his work on Hemorrhage, states, that in one of his Experiments, the Blood taken from the carotid of a Horse, immediately after its division, began to coagulate in *five minutes and a few seconds*, while a portion received when the animal became feeble, caked in *3½ minutes*.

In Dr. Leacock's Thesis "De Hemorrhagia," many experiments on transfusion are related. In one of these, when a Dog had been repeatedly

On a subject which obliges me to oppose my Experiments to those of a Practitioner whom I highly esteem, it is proper to state, that the inquiry was commenced without prejudice; or, if I had any bias, it was to the "Observations" of Mr. Hey: that truth alone was the object of my research, and, that by far the greater number of my experiments were conducted in the presence of competent spectators, whose testimony supports my opinion.*

Since the preceding paragraph was written, I have had the opportunity of conferring with Mr. Hey on the subject. Being informed of the different result of our experiments, he repeatedly expressed a desire for our jointly re-examining the point in dispute, and I was equally willing to accept the offer. Mr. Hey's experiments in reference to the comparative periods of coagulation were made only on the blood of sheep: it was determined therefore, that to these animals, our

bled, till the greatest degree of debility was induced, the author remarked, that the Blood coagulated instantly on its effusion; "*statim coegit.*"

* Besides the assistance of several friends who were casually present, almost all my experiments were conducted with the assistance of my pupils, Messrs. Brearey and Corsellis, whose professional knowledge prevented their being deceived in the results.

observations should be confined. He had also received the Blood in half pint glasses, and though I did not conceive this mode free from objection,* they were used in the first experiment.

Present, Mr. Hey, Mr. Wm. Hey, three of their pupils, and one of my own.

EXP. 1.

Blood was received in three glasses, the periods of coagulation were not minutely noted, but it was *universally admitted that concretion first took place in the last received blood, and latest in the first.*

EXP. 2.

Three Cups were filled with Blood issuing from a wounded Sheep. Here also the periods of receiving the Blood were not remarked, but the interval between each was fully half a minute; Mr. Hey observed, that the first concreted at the same moment with the third, and this he conceived in some measure, to support his opinion. But it was remembered that a *minute elapsed between*

▪ It appears requisite, that the size of the vessel bear a relation to the quantity of Blood in the subject of experiment. If the animal be small, or have but a minor portion of the vital energy, there will be danger of the Blood at the bottom of a large glass, assuming the concretory change before the vessel is full.

the filling of the two Vessels, and that, consequently, the result was in favour of coagulation's commencing latest in the first-received Blood. It was also remarked, that the third Vessel did not contain the Blood of an Animal in the greatest stage of reduction, for the Sheep lived more than a minute afterwards. As however Mr. Hey seemed unwilling to admit the conclusion, a third Experiment was instituted, and the greatest care taken, to render it's result accurate and decisive.

EXP. 3.

Three Cups were filled at the interval of half a minute. One of Mr. Hey's Pupils took the first, and with a minute Watch in his hand, accurately noticed the time of its being filled, and the precise period of concretion. Another of Mr. H's pupils took the second, and observed the coagulating process with the same attention; and Mr. Wm. Hey, took the third,

	<i>m.</i>	<i>s.</i>
1st. Cup began to coagulate in	2...	10
2 ".....	1...	45
3 ".....	0...	55

The result was too conclusive to admit of doubt, and Mr. Hey declined any repetition of the Experiment.

Nor was it on the Blood of animals alone, that my observations were made. In bleeding the human subject, I likewise found that the first flowing blood had the least disposition to coagulate.

EXP. XVIII.

From the arm of a female labouring under fever, Blood was drawn to the amount of a pound and a half; a portion of which received in a tea-cup on the *first effusion*, remained fluid for *seven minutes*; a similar quantity taken immediately before tying up the arm, was caked in *3 min. 30 sec.*

EXP. XIX.

A man, the subject of incipient enteritis, lost about a pound of blood. Of two portions received as in the preceding experiment, the *first* began to coagulate in *seven minutes*, the *last* in *four*.

EXP. XX.

Blood was taken from the arm of a young man labouring under an hepatic affection, combined with fever. A gentleman standing by, was requested accurately to mark the time, at which the vessels were filled, and that at which coagulation took place. On subtracting the periods, the result was as follows:—

	m.	s.		m.	s.
In the 1st. Cup ...	12	25	In the 4th. Cup...	10	5
2	12	0	5.....	8	20
3	11	20			

The vessels were three and four oz. Gallipots.

So *regular* an acceleration of concretory process in proportion to the reduction of the vital powers, is a matter of surprise, nor can it be expected frequently to occur. It is observable that the former periods approximate much more than the latter, and it is probable that if the depletion had been continued, the sixth cup would have concreted in little more than five minutes, and the seventh, in two or three.

Besides these observations, many more might be adduced, if it were necessary, to prove that a *state of diminished tone is most favourable to the concretion of the Blood.**

A tendency to deliquium has also a considerable effect. When concretion has been noticed to take place in five minutes, the occurrence of faintness, I have remarked, has immediately effected such a change in the state of the Blood, as to induce this process in two minutes, and when ninety seconds were required, deliquium has instantly changed the period to 40.

* Since the first check to bleeding is given by the formation of coagula on the mouths of the vessels, we should find, were this process to take place in the inverse order, that Uterine Hemorrhages would almost always prove fatal. Happily however, this not the case.

CHAP. V.

ON THE CAUSE OF THE BLOOD'S COAGULATION.

THIS is an inquiry, which, although it has given rise to various conjectures, and many ill-founded hypotheses, has been the subject of philosophic research. Hewson, especially, has paid particular attention to this interesting study, and merits the credit of having prosecuted the investigation with science and with ardour.

1. Among the opinions held on the subject, some have believed, that the fluidity of the Blood is preserved by the heat of the body, and its concretion produced by its removal into a colder temperature.* The experiments on heat and cold, detailed in the preceding chapter, and the remarks to which they gave rise, will easily confute an idea so devoid of observation. It is well known, moreover, that Blood may be frozen and thawed without the occurrence of coagulation.

* Hippocrates, on observing the victims slaughtered for sacrifice, remarks,—*οταν σφαξη τις ιερειον τεως μεν αν θερμον η, υγρον εστιν το αιμα . επειδαν δε ψυχθη επαγη.*—*De Carnibus.*

And in animals of low temperature, as the Tortoise, if cold produced the concretory process, no circulation could be maintained.

2. The Coagulation of the Blood has been attributed to the action of the atmospheric air. Hewson having included a portion of the Jugular Vein of a living animal between ligatures, admitted air in contact with the confined Blood, and finding concretion to occur in quarter of an hour, he inferred air to be a strong coagulant.

The experiment I attempted to repeat, but found a difficulty in effecting it with success. Besides, were a number of such trials accordant in the result which Hewson states, his inference would not still be established; since other circumstances necessarily interfere with the effect; the air is applied to the vessels, and may in a greater or less degree, render torpid their vital energy; the current of the Blood is no longer maintained, and if his conclusions on the subject of *Rest* were admitted, the loss of motion would claim a considerable share in that result, which he here attributes to the action of the atmospheric fluid. As these subjects, however, will be discussed in the succeeding sections, I proceed to state

a few observations, which *prima facie*, support the opinion of the contact of air's being the cause of coagulation.

(1.) It has been before remarked, that the Blood concretes readily in proportion to the surface over which it is spread; and this effect might naturally be attributed to the air, to which it is so freely exposed.

(2.) In that appearance, which is termed the buff-coat; it has been noticed, that the quantity of size is greatest when the Blood has flowed in a copious stream; and since the formation of this tunic depends on the slowness of coagulation's allowing the red particles to subside, it seems probable, that the free admission of air which takes place when the Blood trickles down the arm, induces its more speedy concretion.*

EXP. XXI.

A vial was filled with the Blood of a stuck sheep, and the cork immediately applied.† During the day it remained an uniform mass, but next morning the Serum was completely exuded.

* See the last chapter in loco.

† The air in this case could not have been wholly excluded, and hence, if even concretion had taken place immediately, the result would have been indecisive

EXP. XXII.

3. Blood was received from the heart of an ox, dead about 20 minutes. It was fluid; but on exposure to the air, concreted in 2 min. 25 sec.)

EXP. XXIII.

Fluid Blood was taken from the heart of an Ox, dead above half an hour. Coagulation commenced in 2 min. 30 sec.

Such observations may be thought to support the doctrine; but the two last, apparently the strongest in its favour, may be proved, on reflection, to aid an opposite opinion. I refer to the same cause, which renders inconclusive, Hewson's experiment,—the state of the vital energy. If (1) and (2) be considered as circumstantial evidence, other observations more powerfully deny the inference. After death the Blood is found coagulated in its vessels. In mortified limbs, coagulation, strong coagulation, is found to have taken place.* In these and similar cases, there could be no direct communication with the atmosphere. On two occasions, when Morgagni witnessed air contained in the blood-vessels after death, there was *no coagulation*, and in a third, but very slight traces.†

* Hunter.

† De sed. et caus. Morb. Ep. v. and xxxi.

Experiment will place the subject beyond doubt.

EXP. XXIV.

Air was forcibly blown from the human lungs into the Jugular Vein of a Horse. The animal almost instantly expired: but the Blood flowing from the body, *though it contained numerous air-bubbles, did not coagulate.*

On reflection, however, it appeared possible that the quantity of Carbonic Acid Gas in expired air, might affect the validity of the conclusion. The following Experiment was therefore instituted.

EXP. XXV.

Into the external Jugular of a Bitch, was injected atmospheric air from a half-pint syringe. The Blood after death *flowed* from the Jugular Vein, but coagulated on its effusion. Fifteen minutes afterwards, the Blood in the vessels, though fully mixed with the injected air, *remained fluid.*

EXP. XXVI.

Blood was received into a vessel, the extremities of which were closed with stop-cocks; and to prevent any connection with the atmosphere, a jet from the vein of the animal, was allowed to pass through, before the lower end was secured;

on the closure of the upper extremity, the vessel was immersed in warm water; yet in a quarter of an hour, *coagulation took place.*

Since, therefore, the admission of air fails to induce concretion (XXIV. and XXV.) and its exclusion to prevent it, (XXVI.) Experiment evinces that *this agent is not the cause of coagulation.*

3. *Rest* has been supposed the cause of this process. To ascertain the truth of this opinion, Hewson instituted experiments on Blood contained in the vessels of living animals. He tied the Jugular Veins of Dogs, and leaving the vessels in their situation, he allowed from 10 minutes to $2\frac{1}{4}$ hours to elapse, before their division. The Blood he found completely fluid on remaining 10 minutes; minute coagula after 15 minutes; and considerably larger clots at the expiration of two hours.* He admits, however, that even these bore but a very small proportion to the fluid mass.

EXP. XXVII.

Immediately on the birth of a living child, I secured part of the Umbilical Cord between

* Crawford also mentions an experiment performed by Dr. Hamilton, in which a portion of Blood was contained between two ligatures, made on the Jugular Vein of a Cat. In an hour, *partial coagulation had taken place.*—*Exper. and Observ. on Animal Heat.*

ligatures, and placed it in water heated to 100° — 110°. At the end of *fifteen minutes*, the vein being punctured, its *contents were found fluid*, and of a natural consistence.

EXP. XXVIII.

The Experiment repeated. At the expiration of *thirty five minutes*, the vessel on division, was found to contain *considerable coagula*, with a small quantity of thick Blood.

EXP. XXIX.

A portion of the Jugular Vein of a large Dog was included between two ligatures, and detached from its cellular connection. The integuments were then laid over the wound in the throat, and the animal kept still. On the opening of the vein, at the expiration of *ten minutes*, *no coagulum* could be observed.

EXP. XXX.

The external Jugular Vein of a Cat was secured with two ligatures. The portion between them contained at the end of five minutes, Blood perfectly fluid. The opposite Jugular was then tied, and here, also, *no marks of coagulation* were perceived, though the vessel was divided at the

expiration of *fifteen minutes*. A Carotid of the same animal was next secured; and after five minutes, punctured. *The Blood was fluid.*

EXP. XXXI.

A similar experiment was made on the Jugular Vein of a Rabbit. A puncture being made *three quarters of an hour* afterwards, *no traces of coagulation* were perceived.

EXP. XXXII.

The experiment repeated. After the lapse of *three-quarters of an hour*, the insulated vein was cut out. *Not the slightest appearance of coagulum* could be discerned.

EXP. XXXIII.

Part of the Jugular Vein of a Dog, with its contents, was included between ligatures, removed from the body, and immersed in water heated to somewhat below 100°. On puncturing it at the expiration of *an hour*, the *minutest coagula* could not be seen.

EXP. XXXIV.

A similar experiment; except that the vessel was not immersed in water. On its division, at the end of *twenty minutes*, the Blood was *perfectly fluid.*

On reviewing these Experiments, we find that where the current of Blood was stopped in living vessels, concretion did not take place in 5, 10, 15, 20, 45, and 60 minutes,* while the reception of the Blood of the same animals in the usual manner, evinced this process to commence in 2—4 minutes. The inference is obvious; *that the loss of motion is not the cause of coagulation.* In reference to the Experiments of the able and ingenious Hewson, a careful perusal of his remarks on this subject, will give reason to believe that his opinion vacillated from the contradictory appearances which his inquiries produced; nor do I conceive, had they been uniform and constant, that the conclusions would have been satisfactory. From my Experiments, it may be conjectured, that his observation of minute coagula after 10 and 15 min. insulation of a vein, originated in some deception†; and the clots which he observed at

* The Experiment (XXVIII.) on the Umbilical Cord can scarcely be deemed an exception. It is objectionable from the Blood's being foetal, and the vessel's probably possessing less vitality than durable veins.

† The solid specks, which he noticed, might possibly have been formed on the outer coat of the vessel, and washed off on its division; or if this supposition be rejected, they might have been produced *after* the exposure of the Blood. An observation in his Work affords some countenance to the latter conjecture. "If they (i. e. the Veins) were opened at the

later periods, may be accounted for on other principles.* Independent, however, of these considerations, Hewson's statements will *support* the conclusion which I draw. If simple *Rest* were the cause of coagulation, this agent and this only would be requisite; and the process would occur as readily when the Blood is at rest in its vessels, as when effused from the body. If on bleeding an animal, concretion take place in five minutes, and on confining Blood in its vessels, the change commence not till 10, the *mere loss of motion, it is manifest, cannot be the cause of coagulation.*

Why does not the suspension of the circulation in Deliquium, Drowning &c. induce coagulation? Though motion is lost, fluidity is maintained, and these occurrences, therefore corroborate the negative conclusion which the preceding observations induced.

" end of 15 min. at first sight it [the Blood] also appeared quite fluid: but
 " on a careful examination, I have found sometimes 1, and sometimes 2 or
 " 3 small particles, about the size of a pin's head, which are coagulated
 " parts of the Blood." During a *careful examination*, it is not improbable that a sufficient time might elapse for the commencement of concretion, since after the inclosure of Blood in a vien, this process occurs in a very short period after its effusion.

* See Sect. 4th.

4. Since fact and experiment so strongly militate against the supposition of either *Air* or *Rest*, simply considered, effecting the Blood's coagulation, we now examine the opinion that the *nervous influence is the source of the Blood's fluidity*, and its loss the cause of coagulation. The nature and functions of the nervous System, however, remaining but very imperfectly developed, we are seldom able in inquiries immediately connected with this intricate subject, to afford direct and forcible proof. I advert, therefore, in the first place to presumptive evidence.

(1.) In the preceding examination of the effects of *Rest*, the veins of different animals were insulated, and it was found, that, although the loss of motion failed to produce concretion, yet that the Blood acquired an *increased disposition* to this change.* In such circumstances, however, the vessels, if not partially dead, were at least so situated, as not to allow the free admission of the nervous influence. On the supposition, then, of this process depending on the loss of vitality in

* *Kellie* also in his observations on the Medical Effects of Compression by the Tourniquet, remarks that Blood drawn from a Vein after the circulation has been for some time mechanically suspended, coagulates sooner than Blood taken on the restoration of this action.

the Blood-vessels, we might conclude, *that concretion would have taken place in a few minutes, had the receptacle been wholly devoid of life* ; but from the vessels possessing a degree of vitality, the Blood did not speedily nor freely coagulate. That *Rest alone, cannot give even a disposition to concretion*, seems evident from the remarks on that subject.

(2.) In 2 instances (XXII. and XXIII.) the Blood was found fluid in the hearts of oxen, 20 min. and half an hour after their slaughter, but coagulated quickly on its removal from these vessels. Vitality, in these cases, must have existed, since the period between the apparent death of the animals, and the examination of the Blood, was too short to admit of the extinction of the living* principle ; and the *circumstance of fluidity's being maintained as long as the Blood was kept in its vital receptacles, and coagulation's shortly occurring on the exposure of this fluid*, strongly supports the doctrine under examination.

(3.) In the Leech which has died soon after suction, the Blood is found concreted, but if it be retained in the *living* reptile, I have found *no trace*

* See galvanic experiments, on the Muscles of the Ox, Page 73.

of coagulation at the end of an hour. It is probable, therefore, that the *Blood's fluidity is maintained by the life of its receptacle.*

(4.) In most of the cases related by Morgagni, in which the Blood after death remained fluid, or but partially coagulated, the patient had died suddenly from some affection of the Brain or nervous system. In such circumstances, it is known, that the contraction of death does not occur, the muscles remain flaccid, and the temperature of the body is long maintained. If these effects be considered as originating from life's still lingering in its tenement, we see cause on the theory adduced for the Blood's tardiness in coagulating.

(5.) On this subject an observation of Fontana's is deserving of regard. He found, that although the coagulation of the Blood *out of the body*, was not affected by commixture with Viper's poison, yet that this substance, when injected into the *veins of a living Rabbit*, produced the instant coagulation of the circulating fluid, and the speedy death of the animal. The effect here stated, could arise, I conceive, only from the shock given to the living principle resident in the heart and its ves-

sels ; and since the preceding remark of Fontana's proves the simple mixture of the poison with the Blood to have no effect on the coagulating process, we must conclude, that *the sudden destruction of the nervous influence induced instantaneous concretion.*

(6.) I have proved by repeated experiments, that coagulation occurs speedily in proportion to the debility of the system. If debility affect primary, and principally the organs of sensorial power, or (to speak, perhaps, more properly) if these organs first lose their excitability, we remark, as a collateral support to the theory adduced, that in proportion as *the nervous influence languishes or dies, the Blood assumes a stronger disposition to coagulate.*

The faintness which occasionally ensues on venesection, has a similar and marked effect : * this occurrence, if I mistake not, can arise only from the *suspension of that supply which the vessels receive from the nervous system, and the consequently increased disposition of the Blood to concrete.*

After thus stating the circumstances which afford plausible grounds for the admission of the Doctrine, I proceed to more decisive evidence.

* See page 54.

Mr. Astley Cooper, to induce me to the examination of a subject, in which, from its intricacy, I despaired of success, kindly favoured me with an account of three experiments which he had made some years ago: but as the statement was merely verbal, I doubt the complete accuracy of my details. The import, however, I believe to have been as follows.

EXP. 1.

Mr. C. received Blood free from atmospheric admixture into the Ureter of an Ox, which had been killed some time before; coagulation took place in ten minutes.

EXP. 2.

Lest the Ureter might be an objectionable vessel, he next tied the Jugular Vein of a living animal with two ligatures, the insulated portion of the vessel remaining in its situation. *The Blood between the Ligatures had not coagulated at the end of ten minutes.*

EXP. 3.

A portion of the Jugular Vein of a living animal was included between two ligatures, and then detached at its lower extremity. At the expira-

tion of four hours, Blood was allowed to enter; and having been secured in this *lifeless vessel*, was found to have coagulated in ten minutes.

These experiments of Mr. Cooper, therefore, favour the opinion of the loss of the nervous influence being the cause of the Blood's coagulation. To ascertain what effect the vital, or lifeless state of the Vessel has upon the Blood's coagulation, I made the following experiments.

EXP. XXXIV.

A portion of the Jugular Vein of a living Dog, was included between two ligatures, removed from the body, and immersed in water heated to 98°. At the expiration of an *hour* the Blood which it contained was found *fluid*.

EXP. XXXV.

To a long portion of the Jugular Vein of a Dog, killed two days before, were affixed, brass stop-cocks, and the whole immersed for a short time in water heated to 90°—100°. The external Jugular of a living Dog, was then laid bare, and after its puncture, one stop-cock was inserted into the opening. A small current of Blood having been allowed to pass through the lifeless vessel, in order to preclude the possibility of its containing

any thing but the subject of experiment, the lower stop-cock was turned, and shortly after, the upper. The vessel, with its contents, was then immersed in the heated water, the temperature of which was maintained for a quarter of an hour. At the end of this period, the vessel being taken out and punctured, or rather cut, over a white plate, the *Blood was found firmly coagulated.*

These experiments evidently lead to the same conclusion as those of Mr. Cooper. *Blood confined in a vessel, which, from the time, vitality could not have deserted, did not coagulate in an hour; while that inclosed in a lifeless vein, was firmly concreted in one-fourth of that period.*

Two experiments of different character will now be stated: but it is proper to remark, that they were made on the supposition, that a part of an animal killed by violence retains its vitality for the space of about four hours, a fact of which I was assured on the most respectable authority.

EXP. XXXVI.*

A considerable portion of the Jugular Vein of a Dog, was detached from the surrounding parts;

* In this, and the preceding experiment, the mode of operation is minutely detailed; that should there exist any deception or impropriety, it may not pass unnoticed.

a slip-ligature affixed to the upper extremity, and on the emptying of the vein, a firm one tied round the lower. The integuments, were then brought over the wound, secured with a suture, and supported with a handkerchief, bound round the animal's neck. He was then set at liberty, appearing in no respect affected by the operation. Four hours and a half afterwards, the wound was re-opened, the slip-knot loosed, and the Blood allowed to enter the vessel. Immediately on its being filled, a firm cord secured the upper extremity, which had been before tied with a slip one. *At the end of ten minutes*, the insulated vessel was cut out and punctured, *but not the least trace of coagulation could be perceived.*

EXP. XXXVII.

A portion of the Jugular Vein of a Sheep, killed *an hour* before, received Blood from another Sheep; the influence of other agents being excluded by the same means as those used in XXXV. After remaining in the temperature of the animal for *half an hour* the vessel was cut, and a *firm coagulum found.*

If the doctrine of vitality's continuing for four hours in a part separated from a living animal

were correct, the vein in the last experiment was alive, and yet concretion took place: consequently, the loss of vitality in the vessel, was *not* the cause of coagulation. In regard to XXXVI. likewise, if death necessarily occurred at the expiration of four hours after the insulation of the vein, the Blood should have lost its fluidity as readily as if received into any other close vessel; yet *no coagulation* could be observed.

The obscurity involving the subject, caused me much perplexing thought, till I reflected, that the statement in reference to the period of death might be incorrect, and that the ambiguity would be probably resolved, if this process were proved to take place at different times, in different animals.

To ascertain whether or no four hours be requisite for the loss of irritability in slaughtered animals, I procured a small Galvanic battery, and having taken a portion of muscle from the neck of a Dog, recently killed, I subjected it to the influence of the Aura, at intervals, for the space of 2½ hours. The contractions were strong at first, but became gradually weaker till the expiration of that period, when no motion, could be produced.

In another Dog, three hours were necessary for the extinction of irritability, after apparent death; and in a third, no contraction took place at the end of two. Three portions of muscle were taken from a slaughtered Ox; and of these one ceased to contract at the end of 2 hours, another at about 3, but the third was slightly affected by the galvanic stimulus, at the expiration even of 6½ hours.

These experiments fully warrant the conclusion, that irritability is lost at different times after the apparent death of different animals; nay, the last evinces it to cease at different periods, in different muscles of the *same* animal.* If, then, one of the most striking properties of life be so inconstant in its abode and departure, may we not infer, that *to the agency and extinction of vitality in general no definite period can be affixed*. What may be deemed the test of life? If the effects of the galvanic stimulus be any criterion, I conclude, from the preceding experiments, that the *death of parts separated from living animals, occurs at various and incalculable periods*.

*The last strong contraction of Muscle, marks the departure of life, independent of that evidence which the effects of the Galvanic Aura exhibit.

The mode, therefore, of ascertaining the influence of vitality on the coagulating process, is by comparing the effects of vessels undoubtedly alive, or unequivocally dead, on the Blood which they contain. With this intention, the following experiments were made.

EXP. XXXVIII.

A portion of the Jugular Vein of a Dog was included between ligatures, removed from the body, and placed on the table. On its division, at the end of *twenty minutes*, the Blood was found fluid.

The character and result of this experiment, resemble those of many others, several of which have been already detailed, (XXIX to XXXIV.) Suffice it, therefore, to remark, that except in one instance*, I never knew Blood to coagulate in a vessel recently insulated or recently removed

* EXP.—A portion of Vein, included between ligatures, was taken from the neck of a Dog, and immersed in water of the temperature of the animal, at the end of half an hour, the Blood was found *partially coagulated*.

From what fortuitous circumstance this exception arose, I am at a loss to conjecture, unless on the supposition of vitality's having ceased or declined, sooner than usual. My experiments on irritability (Pages 73 & 74,) render the supposition plausible.

EXP.—The next morning the other Jugular of the same animal was treated in a similar manner. At the el^d of half an hour the Blood was found *wholly fluid*.

from *a living animal*; and from frequent examination, I feel confident, that the Blood in such circumstances, will be found to retain its fluidity, till the expiration of, at least, half an hour.

Some observations on Blood, confined in lifeless vessels, will now be stated.

EXP. XXXIX.

The Iliac Vein of a Dog, *killed three or four days before*, was armed with stop-cocks, and received Blood from the Brachial Vein of a Man,* in the mode of Exp. XXXV. On examination, at the expiration of *a quarter of an hour*, *concretion was found to be complete.*

EXP. XL.

A portion of the Jugular Vein of a Sheep *killed four days before*, received Blood from a similar Vessel in a living Sheep, in the mode of XXXV. At the end of *a quarter of an hour*, *complete concretion was found to have taken place.*

These Experiments, when compared with XXXVIII. and XXXIV, strongly support the

* From the observations in the 3rd chapter, it is improbable, that any fallacy should arise from use of Blood from *different* animals, particularly when they are of the same class.—Any objection, however, of this nature, the succeeding Experiments will remove.

Doctrine of the Blood's fluidity depending on the life of its vessels. But to make the contrast more marked and decisive, the following Experiments were made.

EXP. XLI.

Part of the external Jugular of a small Dog was secured with Ligatures, and removed from the body. After remaining in the temperature of about 100°, for half an hour, it was punctured. *The Blood was found perfectly fluid.* A portion of the Vena Cava was removed, and reserved for the next day's comparative Experiment.

EXP. XLII.

Fifteen hours and a half after the preceding Experiment, the *Cava taken from the animal* then employed, was filled with Blood (as in XXXV.) from the Jugular of a similar Dog. On the division of the vessel at the expiration *of a quarter of an hour*, complete concretion had occurred.

Thus we remark, that, for an half an hour, Blood remained fluid in a vein recently removed, while in the lifeless vein of the *same* animal, it was found firmly concreted in fifteen minutes. To what shall we attribute this striking fact, if not to the loss of the vessel's vitality?

On reflecting on the Experiments of this section, it occurred to me, that the brass cocks connected with the lifeless veins, might afford matter of objection. To ascertain whether they had any share in inducing coagulation,

EXP. XLIII.

A length of the Aorta of a Dog, *killed the day before*, received the contents of the Carotid of a similar, but living animal. The elasticity of the Aorta, allowing the admission of the divided Carotid, no extraneous apparatus was required. Preventing, therefore, with my finger, the admission of air, and one of my pupils pressing the lower end of the Aorta, this vessel was quickly filled. My assistant was then directed to squeeze down the Blood so as to empty the vessel; care being taken to exclude the Air. The Aorta was then re-filled, and ligatures being applied, was immersed in water, of the temperature of 100°. After a *quarter of an hour* had elapsed, it was divided, and *complete concretion found to have taken* place.*

* Another Exp. which was made, with reference to this subject, produced a similar result, but as the manner of conducting it was not so completely satisfactory as that here recorded, the detail is omitted.

It is apparent therefore, that the same result takes place when stop-cocks, are not employed.

After the preceding illustrations of the opposite effects which the life and death of the vessels produce on the Blood, a specimen succeeds of that intermediate state in which it is probable vitality is but partially existent.

EXP. XLIV.

A portion of the external Jugular was taken from a Sheep, *two hours and a half after it had been slaughtered.* This vessel was filled with Blood (as in XXXV.) from a corresponding vein in a *living* Sheep. After having been kept in the temperature of the animal for a quarter of an hour it was punctured. *Though most of the Blood was fluid, one considerable coagulum was found.**

The inference drawn from the observations of this section, is obvious. Experiments, in which the greatest attention was paid to accuracy in execution, and honesty in detail, have shewn that Blood retained for the requisite period, is found fluid in a living vessel, partially, or irregularly coagulated

* In Experiments where the vitality of the vessel is dubious, the state of the Blood will also be dubious or irregular. In some cases, it may be found perfectly natural, in others grumous, and in some coagulated.

in a semi-living vessel, and firmly concreted in one devoid of vitality. I conclude, therefore, that *the vital or nervous influence, is the source of the Blood's fluidity,—and its loss, the cause of coagulation.*

Exp. XLV.

A portion of the external Jugular was taken from a Sheep two hours and a half after it had been slaughtered. This vessel was filled with Blood (as in XLV.) from a corresponding vein in a living Sheep. After having been kept in the temperature of the animal for a quarter of an hour it was punctured. Though most of the Blood was fluid, one considerable coagulum was found. The inference drawn from the observations of this section, is obvious. Experiments, in which the greatest attention was paid to accuracy in execution, and honesty in detail, have shown that Blood retained for the requisite period, is found fluid in a living vessel, partially, or irregularly coagulated

CHAP. VI.

CHANGES PRODUCED BY DISEASE.

THE Phenomena of Blood, as observed in a state of health, having been detailed in the preceeding chapters, a contrast must now be exhibited between these Phenomena and those which Disease induces. In a general morbid condition of the system, all the organs are affected; the circulation is frequently most disordered, and the Blood, in consequence, assumes a character dissimilar to that of health. The appearances of this fluid as connected with a morbid state, have long attracted the notice of professional men; and though frequently productive of error from the neglect of due observation, or the indulgence of fanciful hypotheses, have, nevertheless, proved to the intelligent and thoughtful practitioner, an assistant to his judgment, and a guide to successful practice. I am not ignorant, however, of the glowing language of De Haen,* nor of the decision

* "Inversa itidem, mutata, confusa, omnia hæc phænomena, sæpius vidimus." After a recapitulation of his Experiments and observations, (chiefly, however, in reference to the Buff-coat) he concludes. "Quæ si ita se habent, quis inflammationem absentem præsentemve, quis humorum

he formed from repeated experiment. Conscious, like him, that the appearance of the Blood is often fallacious, I would by no means consider it, even with the most careful observation, a certain criterion. But I would ask, of the symptoms which direct our treatment of disease, which is not fallacious? Is not the pulse very deceptive, is it not, as Celsus terms it, "*res fallacissima?*" Do we never find the sensation of pain an uncertain guide to the seat of the malady? Or are we always able, from its indication, to ascertain the nature of the morbid action? Is not the tongue in some dangerous affections clean as in health? Are not, in short, almost all the phenomena which mark the character and degree of disease, falla-

aut coagulum aut dissolutionem, ex solo sanguine determinare ausit? utique is in eodem homine alia V.S. ab alia toto cælo differat; si in eadem V.S. adeo oppositissima sint phenomena; quis inde de natura humorum aut possit, aut ausit, judicium ferre." Pathol. in Rat. Medendi.

On this subject however I am happy in concurring with the judicious remarks of Mr. Hey, who after stating that he believes the appearances of the Blood, "give so much information as to merit our attention, answers the objection which arises from the supposed opposition to other symptoms, which these appearances occasionally assume. "In order to direct successfully our attempt, to remove diseases, every symptom must be attended to, and the method of cure must be guided by the indication taken from the assemblage of them all. An attention to all the animal functions is of importance, though considered singly, they may communicate very little knowledge of a disease, or may even mislead us."

cious when individually relied on? If, then, our practice, be only correct and scientific, when we are guided, by the general assemblage of symptoms, it will not be considered useless, to attempt the addition of a diagnostic mark, or practical direction, even though this mark or direction be liable to error. It should be remembered, moreover, that De Haen, like Heberden and other eminent men who have imbibed similar opinions, had a principal reference in his observations to the Buff-coat, a criterion of all others the most fallacious.*

The following observations, with the reports of others who have been usefully employed on the same subject, will convince the unprejudiced mind, that pathological information may be frequently drawn from the appearances of the Blood, the treatment of disease elucidated, and the requisite energy of the remedies indicated, with considerable precision. Examining, therefore, the peculiarities, whether practical or curious, which characterize morbid Blood, the attention is first engaged with,

* If one test prove fallacious, it should not be concluded that all are deceptive, nor even from the occasional error of any test, should we infer its complete inutility. As Gaubius has justly remarked, "*Regulas generales exceptionibus non tolli, nec adeo ordinarias naturæ in sanis ægrisve leges fortuita quavis aberratione infirmari.*" *Instit. Pathol.*

1. THE QUANTITY of this fluid.

On this subject, however, we have no means of gaining accurate information. But it seems probable that the bulk of the Blood is considerably diminished, in some chronic complaints, from the secretions much exceeding the ingesta: while in acute diseases, though the stomach takes no sustenance, yet the great diminution of the secretion, prevents such decrease in the circulating mass.

In proportion to the deprivation of food, the quantity of Blood is believed to be lessened, and where abstinence is carried to the last extremity, the vessels after death are found nearly empty.*

A full or a small, a hard or a soft pulse, might be considered as indicating the quantity of the

* Diemerbroek relates two such cases;

“*Quod autem in tali casu valde paucus sanguis supersit, illud anno 1656 mense Novembri, in Theatro anatomico publice a nobis observatum et demonstratum est, in viro, qui ob cordolium aliquod vitæ pertæsus, se ipsum fame et siti extinxerat: quippe corpore alioqui sanus existens aliquot septimanis nihil omnino edere, ac ne guttam quidem bibere voluerat. In hoc subjecto nullæ venæ meseraicæ, intercostales, aliæque minores conspici poterant, propterea quod inanitæ essent, atque ex ipsa vena cava vix trium cochlearium sanguinis mensura effluxit, et arteria magna omnino vacua inventa fuit. Similiter, anno 1660, mense etiam Novembri, hominem quendam, ob summam et diuturnam inapetentiam longa inedia emanciatum, et tandem mortuum, in eodem theatro nostro publice prosecuimus; neque venas et arterias mirabiliter inanitas invenimus, ita ut ex vena cava vix duo cochlearea sanguinis effluerent, ex arteria aorta nihil.*” Opera.

circulating fluid, were we not aware that the sensation denominated pulse, is produced by pressure on the vessels, and its varieties by the state of these vessels, rather than of the Blood which they convey.

2. THE COLOUR OF THE BLOOD is observed to be changed in some diseases. Long-continued hemorrhages render it much more pale; and frequently-repeated bleeding has a similar effect; this arising from the red particles being of less easy reparation than the other constituents.*

Amenorrhœa, it is said, makes the Blood more pallid; but this circumstance I have never observed on performing venesection in such cases.

On some occasions, a peculiar ruddiness is remarked; the stream from the arm, instead of purple, being of a dirty red. This I chiefly found in robust and muscular men. I have never noticed,

* “Atque interdum sanguis profluit, interdum simile aquæ quiddam, in qua caro recens lota est.—Cels. Lib. iv.

A case of long-continued hemorrhage is related in the Edin. Med. Essays, and referred to by Mr. Hey, in which the Blood had a similar appearance.

“Purpura sanguinis justo pallidior, loturæ carniū similis ex defectu nascitur matriæ rubræ.”—Gaubii *Pathol.*

In a case of Purpura Hæmorrhagica, Dr. Whiting found the red Particles to be in the proportion only of 40 to 1000, while the healthy ratio he states at $\frac{160}{1000}$.—*Thesis de Sang. Ægr.*

however, any variety in the coagulation or other property of ruddy blood.

Hunter, states that he has often seen the stream from venesection to be of a florid hue; but it does not appear that this variety was connected with any peculiarly morbid affection of the system. Lately, in bleeding a youth in small-pox, I remarked the Blood of the Cephalic Vein to have as scarlet an aspect as that from an Artery.

Old authors often notice the black or livid appearance of the Blood in malignant Fevers. Though the colour of this fluid exposed to the air, so much depends on its degree of oxygenization,* as seldom to afford an index to the nature of the malady or its appropriate treatment; yet it may be remarked, that the light hue is chiefly observed on the surface of the Blood taken in the sthenic state. The cause is apparent on reflecting, that in this condition of the system, coagulation does not speedily occur, and hence the Blood is longer exposed to the influence of the Oxygen of the atmosphere.†

* Crawford has shewn that *temperature* has also a considerable effect on the colour, the shade of Venous Blood being deepened by cold, and heightened by heat.—*Experiments on Animal Heat*.

† The serum prevents the action of the air on the coagulm, and consequently when it is quickly effused, the crassamentum has less of the scarlet hue.

3. THE TEMPERATURE OF THE BLOOD has been known to be materially altered. In febrile diseases, the heat of the Blood, as well as that of the body, is sometimes augmented several degrees;† while, on the contrary, in some affections, the Blood, as it flowed from the vein, has produced a marked sensation of cold. Of this curious circumstance, Morgagni has recorded several instances, in one of which, the patient compared the feeling, produced by the stream on the arm, to that of ice. In most of these cases, there existed some affection of the nervous system. Mediavia, as quoted by De Haen, states the circumstance of a woman, whose Blood, from the testimony of her surgeon, was quite cold as it flowed from the vein. The Catamenia, likewise, was always attended with a similar sensation. The patient laboured under what Mediavia terms “*suppressio ac suffocatio pulsuum*.” A like phenomenon presented itself in September, 1818. On bleeding a pregnant female, the stream during the whole period of its continuance produced a chilly feel both on my finger and the patient’s arm. Having no thermometer at hand, I could not accurately note the degree of

† Currie on the Cold Affusion.

cold ; but the temperature marked by a stream of water, which produced a similar sensation, was 68°. The case was attended with no remarkable symptoms, the patient suffering only from headache, and the ailments common to her condition.

4. THE SPECIFIC GRAVITY OF MORBID BLOOD differs from that of the healthy fluid. Dr. Whiting found it less than the standard in seven cases which he examined. These appear to have been all of the acute character, though the patients differed in age and constitution. The lowest specific gravity he states to have been 1041·8. In five of six instances, which afforded me the opportunity of examining this subject, the results, though similar in nature to those he has recorded, were very different in degree:—

(1.) 1022·0

(4.) 1037·3

(2.) 1029·1

(5.) 1037·4

(3.) 1044·7

In a 6th case, the subject a Maniac, it was as high as 1095·7. With this exception, however, the inquiry, as far as it has been prosecuted, leads to the conclusion, that *disease diminishes the specific gravity of the Blood* ; but since the diminution has not been proved to have relation to the degree or

character of the morbid action, no practical direction can be drawn from our statements.

5. The COAGULATION is much affected by a morbid state. In chap. IV. page 47, the fact of the Blood's speedy concretion, in proportion to the weakness of the system, was particularly stated; and the subject was the more regarded, from the practical inferences which it induced.* On this topic, therefore, I will not enlarge, merely contrasting the following Experiments.

EXP. XLV.

A vein was punctured in the arm of a young person labouring under Pneumonia: Pulse 120, and strong. Coagulation did not commence till the expiration of *eight minutes*, though, from the smallness of the vein, the Blood *trickled guttatim*.

EXP. XLVI.

From the arm of a stout young man, affected with Urticaria, half a pound of Blood was taken. A portion received in a small cup, did not coagulate till the expiration of *13 minutes*.†

* Dr .G. Fordyce states in his Practice of Physic, that "the inflammatory Diathesis, is marked by a hard, and for the most part, a strong, full, and frequent pulse; the Blood, when taken from the arm more fluid, and continuing longer fluid."

† So long a period is not usual; nor is it here adduced as an example of a general rule; concretion commonly taking place in healthy Blood at the end of 5—8 minutes.

EXP. XLVII.

A small cupful of Blood from a young woman in the last stage of Phthisis Pulmonalis: Pulse 125. *In four minutes* coagulation had commenced.

EXP. XLVIII.

A Female, aged 33, pallid and emaciated, yet labouring under febrile symptoms, lost about half a pound of Blood. Coagulation commenced in 4 *min.* 10 *sec.* in a cup taken soon after the vein was punctured.

The marked disparity between the periods of coagulation, when the system is under the influence of active inflammation, (XLV.) or remains unbroken by disease, (XLVI.) and when the vital powers are reduced, as in XLVII. and XLVIII., clearly points out the importance of the subject in a curative view. Whoever pays attention to the circumstance, will, I am persuaded, accede to the opinion, that *the speedy occurrence of concretion on the effusion of Blood, affords a reason sufficiently cogent for the discontinuance of depletory measures.*

Blood has been occasionally taken where the vascular action was preternaturally high, yet the system considerably reduced; XLVII. and XLVIII. are instances. In these cases, *increased*

action was combined with *diminished power*; the former tending to retard, the latter to accelerate, coagulation. Hence, though some variety occurred, I generally found concretion, under such circumstances, to take place in a medium time.*

The *completion of coagulation*, does not observe the same regularity as its *commencement*. Of six experiments, tending to ascertain the speedy effusion of serum as influenced by the tonic or atonic state, four favoured the idea of the tardy separation of the Blood's constituents in cases of extreme debility, but the others gave a contrary impression. Two observations, however, since made, support the former opinion.

EXP. XLIX.

A Puppy was killed by the division of the Jugular vessels. The Blood received in a small cup, had separated its serum at the expiration of 24 hours in the proportion only of 29 in 248; or as 10 to 85.

EXP. L.

A poor woman, the subject of Apoplexy. Blood with a loose coagulum and a deep fibrous coat. In two days,—*serum* 27, *crass.* 470; or as 10 to 174.

* For the illustration of my ideas on this subject, see section 7th, of this chapter.

It is probable, therefore, that perfect coagulation occurs most readily in a healthy condition; and that both inflammation, and weak vascular action retard the due exudation of the serum. It would seem from the observations of Huxham and others, that, in some complaints, the Blood does not, in any degree, separate its fluid. In malignant petechial Fevers, he says, “the crasis is so broken as to deposit a sooty powder at the bottom of the vessel, the upper part being either a livid gore, or a dark-green, and exceedingly soft jelly.” In some scorbutics, also, where a disposition to hemorrhage exists, he remarks, that the Blood does not coagulate, but becomes grumous. De Haen also saw the Blood in a dissolved state. Richerand* states, that the venous Blood which issued from the stump of an old Man, whose arm was amputated, was similar to a weak dye of Logwood, entirely dissolved, purple, and never underwent the true coagulating process. Neither, according to the statement of Authors, does the Blood coagulate in the Plague.

Hewson saw Blood taken from a Woman, the subject of fever, ensuing on parturition, which did

* Elem. Phys. by De Lys.

not separate into serum and crassamentum. Cases of non-coagulation are recorded also by Morgagni, Fernelius, and Schwenke. Mr. Wm. Hey, in his work on Puerperal Fever, notices two instances of fatal termination, in which the Blood was found, on venesection, to be in a dissolved state.

A cupful of Ox's Blood, I once observed, which, instead of coagulating, formed an orange-coloured turbid fluid, with dark grume at the bottom. In two cases, also, I remarked the coagulum to be dissolved in the serum, even after the latter had completely exuded. In the Blood of a very young animal, the serum, on the second day, was distinct from the crassamentum, but on the third, they formed one dark thick fluid. This circumstance was probably the effect of incipient decomposition, for the Blood soon emitted a fetid smell. Blood drawn from a young Female affected with severe abdominal pain, separated its serum, as usual, but on the fifth day, the coagulum and fluid were found an uniform mass. This occurrence, however, I was inclined to attribute to some fortuitous cause.

In my observations on the Blood of animals, I frequently remarked, that, if the serum of the

young were not poured off from the crassamentum, it shortly became turbid, apparently from the mixture of a portion of the coagulum.

In animals killed by certain poisons, the Blood does not coagulate; as in Ruminants destroyed by Arsenic.* Neither does this process take place in deaths from lightning, canine madness, the venom of serpents, blows on the stomach, and mental emotions, nor in animals killed in the chase.

When extravasated in a living part, the Blood generally concretes. Hunter notices, that in the mortification of a Member, it is found strongly coagulated. Haller has a remark, that a concreted tremulous jelly has been seen in the vessels of a patient dying from fever; and even in the veins of a living person.†

6. THE FIRMNESS OF THE COAGULUM OF BLOOD has been considered a distinctive mark of the tonic state of the system; its great tenacity, a characteristic of inflammation; and its looseness, a sure proof of debility. During inflammatory action, the fibrine of the Blood is known to be considerably augmented;‡ and since this action is

* Brodie, Phil. Tran. † Primæ Lineæ.

‡ Dr. Whiting, on examining the Fibrine of morbid Blood, generally found it to be increased, and especially where the sily tunic was present.

generally produced by nature, for the reparation of some injury, we readily perceive the reason of such increase. Its effects on the coagulum, as we might *a priori* expect, are found to be great, the crassamentum being larger, and much more dense in its texture. Reflection on this circumstance, has generally a considerable effect in the treatment of disease; most practitioners thinking themselves authorized to repeat venesection, where the first-drawn Blood has a firm crassamentum, and obliged to desist from the antiphlogistic regimen, when the coagulum is readily broken. These marks, however, ought not to be wholly relied on. In acute diseases, it is frequently found that the serum is slowly exuded; and hence, unless a due time elapse before examination, the coagulum is soft from the serum it contains. Here, upon the general principle, the practitioner would desist from further evacuations, concluding the system to be greatly reduced. Sometimes, also, from the adhesion of the coagulum to the side of the vessel, from the kind of vessel,* or other cause, the sepa-

In two cases of acute Rheumatism, its proportion to the whole Blood was as 7 to 1000, and in a third, 9·7 to 1000; although the ratio in health, is but 1—2 to 1000.

* Mr. Hey properly objects to the use of pewter porringers, where it is desirable to observe the changes of the Blood. Either glazed earthenware or glass, ought certainly to be preferred.

ration of the serum is prevented for many hours, yet, on the removal of such attachment, or on the division of the coagulum, the serum is effused, and the crassamentum becomes firm. The size of the vessels has also a considerable effect on the exudation of the serum, and consequently, on the density of the remaining coagulum. The fluid of Blood received in a basin, is usually in greater proportion than that contained in a small cup, and of course, the cake in the latter, is looser than that of the former. If, however, on the *division of the coagulum, at the expiration of from eight to twenty-four hours, there ensue no considerable effusion of serum, and the crassamentum remain extraordinarily firm*, I believe that *further depletion is fully warranted*. Dr. Langrish, in his experiments on morbid Blood, used a glass tube, resembling that of a thermometer, for ascertaining the tenacity of the crassamentum. He placed the Bulb on the coagulum, and adding quicksilver, found what quantity was necessary to penetrate it. His glass was graduated, and hence he could the more readily notice “the degrees of cohesion.” From his observations, it appears, that in acute Fevers, including cases of inflammation, the average of the tenacity was about 36;

that of tertian fever was 23 ; and that of quartans, 17. He further remarked, that Blood drawn from three healthy young men, had crassamentum of but 9, 10, and 12 degrees of cohesion. Dr. G. Fordyce remarked, that the Blood coagulates into a firmer or looser mass, generally in proportion to the strength of the system.* The same opinion is also repeatedly advanced by Mr. Hey.

In acute maladies, therefore, it is evident, that the coagulum is generally dense. We frequently, however, observe much benefit derived from bleeding, even when the crassamentum is soft and yielding; nor should we, in such cases, hesitate to repeat the depletion, if other circumstances indicate its propriety. Dr. Watt, in his cases of Diabetes, remarks, that great advantage accrued from venesection, though the coagulum was loose and black ; and that on repeated evacuations of Blood, the crassamentum became much firmer, and of a more natural hue. But it is by no means to be inferred, that inflammation, or acute disease, exists in every case where bleeding proves beneficial.

7. THE PROPORTION OF SERUM AND CRASSAMENTUM, affords an important evidence of the state

* Elem. Pract. Physic.

of the system. In most of the experiments of the 4th chapter, regard was had to this circumstance, but as the observations referred, not to the ratio at repeated bleedings, but to that of Blood taken at the same hemorrhage, the results were not conclusive. On examining, however, the statements in the Appendix, it will perhaps be admitted, that even in slaughtered animals, the serum is *usually* in a proportion inverse to the strength of the system.

The two following observations support the general inference, that the *serum is relatively increased during the continuance of bleeding*: and it is surprising how great a change in this respect, the lapse of a minute produces.*

* My friend Dr. Weatherhead (now of Upper Montagu-street, London) suggested the following query. "Does not the abundance of serum in the last drawn cup, arise from the immediate effect of the bleeding in rousing the energies of nature to absorb serum from the different cavities, and thus occasion the fact you have remarked? If this be the true cause, it will likewise account for the benefit derived from, and authorize the detraction of Blood in both sanguineous and serous extravasations, wherein the strength has not thereby been materially diminished. This absorption ought also to be more obvious in health, where nature acts, unfettered by diseased associations." The circumstance must, I conceive, originate either in the cause which Dr. Weatherhead suggests, or in the greater disposition to concretion which the Blood assumes, when the system is reduced. The latter opinion is least probable, for if the increased proportion of fluid in the last received Blood, arise merely from the speedy contraction of the crassamentum's ejecting a greater quantity of serum than the first-drawn, this substance would be firmer in the last than the first. But since the fact proves the reverse, we may infer, that *during hemorrhage, the ratio of serum is actually augmented.*

EXP. LI.

About 1 lb. of Blood was substracted from the arm of a muscular man, labouring under Angina Pectoris. It was weighed three days afterwards :

	Ser.	Cras.	Ser.	Cras.
1 Cup had	160.....	360 or as	10 to	22.5
2	420.....	594	10 ...	14.1
3	418.....	736	10 ...	17.6*

Great faintness occurred on filling the third cup.

EXP. LII.

In XX. the proportions were found to be in

	Ser.	Cras.	Ser.	Cras.
No 1	260.....	335 or as	10 to	12.8
2	500.....	520	10 ...	10.4
3	400.....	526	10 ...	13.1
4	440.....	500	10 ...	11.3
5	450.....	536	10 ...	11.9

No disposition to Deliquium.

However the fact may be as to the proportion of the Blood's constituents during hemorrhage, certain it is, that frequent bleeding diminishes the quantity of the crassamentum in the circulatory system, and equally certain, that, in most diseases of the atonic character, and in a reduced state of the vital powers, the quantity of serum is large.†

* Mr. Hey, also, commonly found that the least serum was effused from the first drawn Blood, and most from the last.

† Hence it is evident that the crassamentum is less readily produced.

In the 2d chapter it was stated, that the proportion in health, is 10 to 13 or 14 of crassamentum. The comparative quantities in morbid Blood will now be adverted to, beginning with maladies where debility formed the principal feature.

EXP. LIII.

In a Dyspeptic case, a small quantity of Blood was drawn at the Patient's request,—a female of pallid countenance, spare habit, and health long impaired. The Blood looked thin, as it flowed, and assumed a partial coat of Size. *Ser.* 250, *crass.* 285 ; or as 10 to 11·4.

EXP. LIV.

Blood was taken from the Temporal Artery of a Man, whose constitution was feeble, and employment sedentary. His complaint was Ophthalmia, but, with this malady, the system did not appear affected. On weighing the Blood two days afterwards, *the serum and crassamentum were very nearly equal in quantity.*

EXP. LV.

Blood was taken from a Woman, aged 46, afflicted with the Hemiplegia : *serum*, 420, *crassamentum*, 520 ; or as 10 to 12·3.

EXP. LVI.

Blood was drawn from the arm of an emaciated Female, long the subject of Ascites.* At one venesection, *the serum was to the crassamentum*, as 10 to 11.2; at another, as 10 to 12.9.

EXP. LVII.

In XLVII., a young woman in an advanced stage of Phthisis, the Blood, two days after its subtraction, weighed, *serum*, 345, *crassamentum*, 540, or as 10 to 15.6.

In these cases, with the exception of the last, the serum was in a proportion greater than the healthy standard. Dr. Whiting seems to doubt the truth of this circumstance's generally occurring in a marked degree, in cases of atonic disease. From my own observations, I am inclined to believe, that as much depends on the robust, or reduced state of the system, as on the character of the malady. Where the animal frame is weak

* Though not connected with the subject of this inquiry, I may be permitted to remark, that bleeding is not unfrequently beneficial in Hydropic cases. The statements and reasoning of the able and ingenious Parry, supported by careful observations, evince the diseases to be almost ever connected in one stage or other, with Inflammatory or increased Vascular action; and hence the utility of subtracting at intervals, small quantities of Blood. The case alluded to, (LVI) was under the care of my friend, Mr. Battye; but in a patient of my own, the subject of Anasarca, I recently found the best results from the depletory treatment.

and delicate, the proportion of serum is usually large ; when the disease is unattended with Fever or Inflammation, the proportion is increased. On the combination, therefore, of these causes, the *Blood will generally be found preternaturally fluid*. If, on the contrary, the system be strong and robust, the serum is comparatively small ; if the affection be of the inflammatory character, the serum is considerably diminished. On the combination, therefore, of *these* circumstances, we may expect the *fluid to form but a small proportion to the solid parts of the Blood*.

It is by no means an unfrequent occurrence, however, for debility to be combined with acute disease. In this case, according to the opinion I have stated, are two principles or causes, opposite in their nature, and tending to produce contrary results, and in proportion as the one exceeds the other, shall we have a diversity in the quantity of serum.

Experiment LVII. affords an illustration of my ideas. Here there was *increased action* conjoined with *diminished power* ; a pulse ranging from 120 to 140, and a constitution greatly reduced, the former tending to lessen the relative quantity of

serum, and the latter to augment it. In consequence, we find the serum to be in proportion of 10 to 15 while, had there existed no principle to counteract the agency of the high vascular action, it would probably have borne a relation only as 10 to 20—25.

Some observations will next be adduced to shew the proportions in the tonic state.

EXP. LVIII.

Two or three ounces of Blood were taken from the Arm of a Girl, aged seven, labouring under Pneumonia. *Serum*, 166, *crassamentum*, 460, or as 10 to 28·2.* Leeches were applied to the breast the succeeding day; and on the third, about 4 oz. of Blood taken from the Arm. *Serum*, 819, *crassamentum*, 1124, or as 10 to 13·7.

EXP. LIX.

Blood was taken from the Arm of a Woman, who had been of a robust constitution, but was now affected with a mild Gastritis, or what might be termed Gastric Fever. No buff. *Serum*, 80, *crassamentum*, 225; or as 10 to 28·1.

* In this, as well as the succeeding cases, care was taken that the serum had duly exuded.

EXP. LX.

In a case of decided Enteritis, on the second bleeding, and after the inflammatory action had been much reduced; the proportions in two vessels were found the succeeding day, to be, in the first-received, *serum*, $24\frac{1}{2}$,—*crassamentum*, 39; or as 10 to 15.9; the second, *serum*, $22\frac{1}{2}$,—*crassamentum*, 42; or as 10 to 18.6.

EXP. LXI.

Continued Fever. A Female, aged 26. *Serum*, 325,—*crassamentum*, 520; at a second venesection, *serum*, 250,—*crassamentum*, 410; or as 10 to 16.2.

EXP. LXII.

Incipient Enteritis. A Man, aged 58. Blood buffed and cupped; *serum*, 680,—*crassamentum*, 1,180; or as 10 to 17.3.

EXP. LXIII.

Acute disease of the heart. A Man, aged about 30; *serum*, 169,—*crassamentum*, 237; or as 10 to 19.9.

These cases, it is hoped, sufficiently evince the increased quantity of *crassamentum* in febrile and inflammatory action.

As manifesting the necessity of this substance's being in a moderate quantity, I refer, particularly,

to LVIII. From the first attack of the complaint, to the second venesection, the child was in great suffering; but no sooner was the proportion of crassamentum reduced to its healthy degree, than the pain was greatly relieved, and the malady, in 24 hours, completely removed.

From Langrish's experiments, before referred to, it appears, that the average of the relative quantities, was 10 *serum* to 33 *crassamentum*, in acute Fevers; 10 to 25 in Tertians; and 10 to 16 in Quartans.*—Dr. Mills, in almost all the cases of acute Fever, which he records, found the serum to be in very small proportion to the crassamentum, though it does not appear that the Blood was weighed.—In some acute maladies, scarcely a drop of serum is exuded; and, what is remarkable, in Asthma, a like circumstance has been observed. Morgan† states the case of a Female, labouring under this disease,—a pulse small and quick, “with melting heat,”—a pound of Blood separated, but 2½ oz. of fluid; and at another bleeding, the crassamentum was scarcely covered

* It seems probable, that in these cases, the serum had not been completely effused; for the proportion of crassamentum in Langrish's statements considerably exceeds that commonly found in similar diseases.

† Principles of Medicine.

with serum. Some months ago, I took about 6 oz. from the arm of an elderly man, affected with Asthma, with a pulse of 140, soft and weak. Next morning, I was surprised to find, the exuded serum not to equal a tea-spoonful; and scarcely did the coagulum, on its division, separate any additional quantity.—While, however, it is maintained, that the crassamentum is usually in a quantity proportionate to the tonic state, exceptions must be admitted occasionally to occur. The following is an example:—

EXP. LXIV

Blood was taken from the Arm of a robust young Woman, affected with Pleuritic symptoms.
Serum to crassamentum, as 10 to 12·6.

Although, therefore, *an increased quantity of crassamentum marks the inflammatory state*; yet, from the absence of this criterion, we should by no means infer, that the depletory treatment is precluded. Repeated observation has convinced me of the decided benefit, which frequently ensues on venesection, even when the Insula bears no inordinate relation to the fluid constituents of the Blood.

But, where this circumstance *does* occur,—where the crassamentum is in a ratio preternaturally large, there I believe bleeding to be required, whatever be the name of the malady or of the symptoms which attend it.

The proportion of fibrine is well-known to be much increased during gestation. Some writers, however, are disposed to consider this circumstance as depending rather on the malady for which blood-letting is performed, than on the pregnant state. But it seems very improbable, that the crassamentum should be augmented to a degree, exceeding often that of the most active inflammation, if the only cause were a pain in the Thorax, or a slight febrile affection. The true cause appears to arise, from the necessity of an additional quantity of the plastic Lymph for the formation and growth of the foetus.

A scanty supply of nutriment diminishes the proportion of crassamentum, and a similar effect is produced by old age. Morgagni* relates the case of a poor man of 60, in whose Blood after death, no fibrous part could be found.

Connected with the subject of this section is the

* Epist. 43.

quantity of water in morbid Blood, as evidenced by evaporation. The analyses of Berzelius, Marcet, and Bostock, in reference to the natural proportion, may be seen in the appendix. Whiting found it in healthy Blood, 770—794 to 1000; while in a reduced condition of the vital powers, it was considerably augmented, amounting even in a case of Purpura Hemorrhagica, to $\frac{862}{1000}$.

8. Of the appearances which the Blood assumes in disease, that most regarded by the Moderns, is **THE COAT OF YELLOWISH SIZE, WHICH SOMETIMES COVERS ITS SURFACE.** This has usually been considered the criterion of inflammation, since its presence is commonly found in acute Rheumatism, Pleurisy, and the like. In these affections, the Blood, on flowing from the arm appears thin, and on remaining three or four minutes, throws off a bluish lymph, which quickly concretes into a dense sheet, much resembling the spurious membranes produced by internal inflammation. This tunic allowing the coagulum to adhere to its inferior surface gradually increases in thickness, till it reach the $\frac{1}{2}$ or $\frac{1}{3}$ of an inch.* In some

* Huxham remarks that in some fevers, he has seen it an inch in thickness.

cases the surface is concave ; and this appearance is considered as evincing a higher degree of acute disease.

It seems probable that a variety of circumstances tend to produce the fibrous tunic. In cases where the mineral, and even vegetable acids, have been freely administered, it has been observed ;* and it will often exist when the system is under any actively morbid influence.

Though commonly found on venous, it appears from Dr. Gordon's statements, that the buff-coat is also occasionally witnessed on the surface of arterial Blood.

Authors had long noticed this appearance without adverting to its nature, or explaining its formation. Gaber† and Hewson were the first who examined the subject with due attention. Instead of a morbid structure, they considered its principal constituent to be coagulated lymph or fibrine. This opinion has since been controverted by Mr. Hey, who maintains, that serum forms by far the largest proportion. The fact is, that the size is found, in some cases to be 9-10ths of it fluid, while in others, it is tough and compact like leather.

* Vide Cruickshanks on Acids in Lues.

† De humoribus animalibus,

In regard to the formation of the buff-coat, Hewson concluded, that it was produced by the *tenuity of the Blood*, allowing the red Particles more readily to sink. That the Blood is attenuated in inflammatory affections, however, is an assertion, which most practitioners, both ancient and modern, are disposed to deny. It is true, indeed, that the Blood, as it issues from the arm, in acute maladies, usually appears thin, and the thinnest often in the most acute. But I am convinced from observation on diseased Blood, that *no real and permanent attenuation occurs in Inflammation.*

The only mode in which, I conceive, the crassitude of the Blood can be diminished, is either by an inordinate proportion of serum, or a preternatural fluidity of the crassamentum. In the two last sections of this chapter, the *tenacity of the coagulum* has been shewn to be *considerably increased* by inflammatory action, and the *proportion of serum greatly diminished*. If, therefore, there exist any attenuation, it can only be before coagulation; and its cause must arise from some unknown change produced in the quality of the old fibrine, or from some variety in the character of the new.

But, since the tenuity of the Blood does not appear to occasion the sily tunic as observed in Inflammation, it may be asked, "Is it produced by an increased specific gravity of the red Particles?" Hewson has answered this query. Having poured into two vials, the serum of Blood which did not assume the buff-coat, he added the red Particles of sily Blood to the one, and the red Particles of healthy Blood to the other: the former did not subside sooner than the latter. He concluded, therefore, that the gravity of the cruor was not the cause.

On drawing blood during high vascular action, I had repeatedly noticed, that the first cup, which remained fluid the longest had the strong size,—the surface of the second had transparent spots, and the third which was soonest concrete, had no such appearance. Hence I was led to infer, that the formation of the buff-coat in such diseases, depends on the Blood's indisposition to coagulate, such tardiness of concretion allowing the red Particles to subside.* This opinion I had formed

* An observation of Dr. Whiting's, which apparently militates against this opinion, ought to be stated. He found a fibrous tunic near the *bottom* of the crassamentum in one case; and this phenomenon to be wholly absent in another, though the Blood remained fluid from 15 to 30

before I noticed Hewson's remarks on the subject. That inflammatory action retards concretion, the preceding parts of this Essay have fully demonstrated, but the cause of this delay, is a point not so easy to determine. Hewson supposes, that the great agitation of the Blood in acute maladies, occasions a more intimate union of its constituents, and a consequently less ready separation.

I bled a patient labouring under Pleurisy, whose pulse, before venesection, was but 80, yet a fibrous tunic was formed. In another, the subject of Enteritis, with a pulse of 84, three vessels of Blood had each a sizzly crust.

Doubting, from these observations, the propriety of Hewson's solution, I am disposed to ask, does the additional quantity of fibrine, acquired by inflammatory Blood, remain for some time unassimilated, and thus retard those changes which it is natural for this substance to assume?

Or rather shall we believe that the vital energy is preternaturally excited in active disease, and minutes.—A remark of Kellie's, however, in his observations on the tourniquet, seems strongly to support the opinion. He states, that when from the stoppage of the circulation, the blood acquired an increased disposition to coagulate, no buff-coat appeared, but that on the removal of the instrument, and the consequently free passage of this fluid through its vessels, the tunic was very conspicuous.

thus by its effects on the Blood Vessels, protracts the period of coagulation?—That a state of nervous, as well as vascular excitement exists in inflammatory affections, will be readily admitted: that this excitement, or encreased nervous influence, affects the Blood vessels in an especial degree, cannot be denied: and since the preceding chapter has evinced the intimate connection of the vital energy with the Blood's coagulation, nay, has shewn the dependance of this process on the life of the vessels:—since, moreover, section 10, of chapter 4, has experimentally proved, the coagulating periods to correspond with the efficiency and reduction of the vital energy:—the conjecture is at least plausible. If this supposition be admitted as a consistent and adequate theory, it will explain, not only the origin of the fibrous tunic in inflammatory affections, but also the facts connected with the relative periods of coagulation, to which so much attention has been paid.

This theory, however, refers only to the formation of the fibrous coat, in maladies of acute character.—In Scurvy, and other diseases of debility, the same appearance is often remarked,

yet the same rationale cannot be admitted. But in these cases, it should be remembered, that the proportion of serum is generally increased, and the Blood, in consequence, becomes *really thinner* than in a state of health.—It appears, therefore, that a facility is here afforded for the subsidence of the red particles, equal to that produced during inflammatory action, by the tardiness of concretion. Hence I infer, that *the buff-coat is produced in acute diseases, by the Blood's indisposition to coagulate, and in those of debility, by its actual tenuity.*

Where the fibrous tunic occurs in cases of debility, the coagulum beneath it, is loose and dark coloured.

This characteristic has been noticed by several authors, but by Mr. Hey, it has been brought forwards in the most useful point of view, and since the rule is general, its precautionary inferences are obvious.

Among the observations made on the sily tunic, it has been noticed, in some cases, to assume a cornelian hue ; this appearance no doubt arising from part of the cruor being entangled in the

Pellicle. Huxham remarked it in Pneumonia Notha, and considered it as an unfavourable omen.—Hewson also saw it in one or two instances.

The sily coat is found to occur in almost all cases where nature is either forming a new substance, or repairing an injury. After capital operations, the Blood always assumes this appearance; and in Pregnancy, especially in the latter months, it is well known to exist.

The practical conclusions, which this subject induces, demand serious attention. The fibrous tunic is admitted to present itself in cases of the greatest vital debility,—cases in which, the loss of but a moderate quantity of Blood, would hurry the patient to his tomb; while this fancied criterion of acute disease, is often, nay commonly, absent in maladies, where severe inflammation is combined with the greatest constitutional strength.* In affections, moreover, which require

* De Haen, *cum multis aliis*, has pointedly made the observation, “Imo in morbis maximè inflammatoriis, in nullo sanguine, quoties cunque misso, aliquoties crusta ulla est.”—*Ratio Medendi*.

“Vidi dum sanissimis hominibus vena secaretur quotannis verno tempore, sæpius crustam adesse. Imo in homine debili, cui ad Hæmoptysin præcavendam, qua laboraverat, sanguis mittebatur singulis tribus mensibus, vidi semper crustam illam adesse. Adfuit ergo talis sanguinis diathesis, licet nulla inflammatio adesset. Et contra in validissimis inflammatoriis morbis aliquando nulla talis crusta in sanguine apparuit.—*Van Swieten in Aph. 384, Boerh.*

the repeated abstraction of Blood, it has often been remarked, that the buff-coat has not appeared on the first or second bleeding, yet on the third or fourth, it has been copiously exhibited. What shall we say, then, of the doctrine of this tunic's characterizing inflammatory action, and warranting the repeated abstraction of Blood,—the doctrine, I mean, unlimited and unguarded? If the observation and the reasoning of the ablest experimentalists had a due effect in forming the opinions of medical practitioners, and influencing their conduct, this dangerous opinion had been long since exploded. But if the admonitions and the warnings of a host of authors deserve no regard, surely personal observation ought not to be neglected, nor reflection wholly abandoned. To examine the aspect of Blood in disease, is not difficult, and to reflect on the fatal effects of the error so frequently exposed, is not useless. If, however, no other consideration will avail, I would urge the counsel of Huxham, “As for those who will neither read nor reason, but practise by rote and prescribe at a venture, I must seriously advise them, at least, to peruse the sixth commandment.”

9. Some changes are produced by disease in the QUALITY OF THE SERUM. Dr. Parr states the albumen to separate sooner, and to be less firm; but in no determined ratio to the nature or the violence of the complaint. Dr. Marcet examined the specific gravities of serum in some affections. In a slight local injury he found it 1024·5; in acute Rheumatism, with a pulmonary affection, 1032·5, and on a repetition of the blood-letting the next day, 1029·8; in a febrile affection, 1029·8; in a case devoid of any inflammatory appearance, 1030·9.

That Fever and Inflammation produce no considerable effect on the specific gravity of the serum, may be inferred from these experiments as well as from the statements of Dr. Whiting. This gentleman found also, that in a patient, greatly reduced by Purpura Hæmorrhagica, the specific gravity of the serum was nearly the same as that of the healthy fluid.

In numerous instances, I have examined this subject, but have not ascertained, as connected with it, any constant law of nature or disease. The cases in which the specific gravity was generally found the greatest, were Pleurisy and other

acute disorders, while, in a debilitated state of the system, it was usually the least. A few examples are subjoined :

Pleurisy,a Female	1038·9
Dropsy, a Man,	1082·0*
Disease of the Heart, a delicate Man,	1012·7
Puerperal Fever, ... 1st bleeding,	1002·6
..... 2d ditto,	1009·8
Inflammatory Fever, a young Female, (serum } of a remarkably deep colour)..... }	1046·9
Acute Rheumatism, a robust Man,	1034·3
Pleurisy,	1036·9
Puerperal Convulsions, for which the patient } had been repeatedly bled,	1014·3
A Chronic Affection, unconnected with organic } disease,	1026·8
In two parcels of serum, taken from a subject of } Fever,—my friend, Dr. A. Hunter, found } the specific gravities to be..... }	1044·0 1053·0

In health, the quantity of saline matter is in very small proportion; but in acute or febrile affections, we find it sometimes increased. In the former state, as well as in diseases of debility, the serum is nearly insipid, in the latter frequently salt and pungent.

As, however, the taste cannot be relied on, I examined the solid contents by evaporation in a few instances, but since the preceding remarks on

* Since I never met with another instance of so high a specific gravity I suspect an error in the calculation.

the specific gravity precluded, in a great measure, the necessity of minute inquiry, the subject was but cursorily regarded. The Experiments† as far as they were prosecuted, led to the conclusion, that the *quantity of solid matter in serum bears a relation to the degree of inflammatory action*. Dr. Whiting's researches, however, ascertained no marked nor regular disparity in the proportion of saline matter.

The *colour of morbid serum* sometimes differs from that of the healthy fluid. In Disease, it is often greenish or straw coloured, and in ardent fevers, orange or bilious, and frequently of a flammeous hue.—The deepness of the tinge generally occurs in acute maladies.

The *coagulation of serum in disease*, has also been examined; but of many parcels subjected to experiment, only in one or two was any difference observable, either as to the temperature requisite for concretion, or in the after-appearance of the mass. In one case it coagulated at 148° and in another at 135°.—These varieties, however, were probably occasioned by some casual circumstance.

† See Appendix.

In regard to the *milky appearance*, which the serum sometimes assumes ; it is doubtful, whether it be an accidental occurrence, or a symptom of disease. It has been frequently noticed by authors.* I have seen it in three instances. One was a man about 27, the subject of Epilepsy, depending on Plethora of the vessels of the head ; the 2d was a Man of 58, affected with abdominal Inflammation ; the particulars of the third case I have forgotten. Three or four similar occurrences, though less in degree, I have since noticed ; but in these, like the former, no peculiarity of constitution, or disease, was remarked. Hewson believed this cream-like substance to be produced by the absorption of Fat. The Epileptic patient, just referred to, was thin and lean : the other man was muscular, and somewhat inclined to obesity.

Hunter thought white serum most frequent in pregnant women ; but neither has he, nor any other author, given a satisfactory account of its formation.—It has been commonly supposed to arise from unassimilated chyle.†

* Hewson in his Treatise quotes many examples.—

In a case recorded in the first Vol. Philos. Trans. one portion of fluid received from a vein, is said to have been *wholly* this substance.

† Many minute inquiries relative to the Blood in disease, might have been made, were I not aware, from the example of Dr. Whiting, that the

10. It may not be improper to notice, that we frequently find **DEPOSITIONS** from the Blood in disease. In inflammatory affections, lymph is often effused, whence preternatural adhesions are formed, and frequently membranous sheets. It is scarcely necessary to mention the other products of Inflammation, as Pus, Ichor, and the like. Flakes of fibrine are sometimes separated from the Blood in acute disease, and evacuated by the secretions.

information, accruing from this research, would have been of little moment, either to Physiology or Pathology. The honest and ingenuous manner, in which this Gentleman has stated the result of researches, no less patiently and accurately prosecuted, induce me to take the liberty of quoting the inferences, which conclude his Treatise.

“ Hæc tandem certiora ex præeuntibus colligere licet :

1mo. Sanguinem e sanis plerumque sanguine ex ægris esse graviolem.

2do. Gravitationem specificam sanguinis de copia materiæ ejus solidæ haud certi quidquam indicare.

3tio. Sanguinis totius gravitationem non semper eadem ratione ac seri augeri aut diminui.

4to. Copiam seri, quod sponte exudet, aquæ in sanguine toto rationem minime præbere.

5to. Aquæ copiam in sero minus quam in sanguine integro tam sanorum quam ægrorum variari.

6to. Quo magis vis vitæ deficit, eo sanguinem esse aquosiolem.

7mo. Fibrinam in omnibus fere morbis, præcipue cum inflammatio adest, augeri ; sed viribus corporis copiam ejus non respondere.

8vo. Májorem eandem esse cum crusta coriacea simul adest ; et quo magis hæc scatet, eo et illam plerumque abundare.

9no. Concretionem sanguinis ex fibrina penderi.

10mo. Quantitatem albuminis et serositatis, tam sanis quam ægris mínime variari.”

Ossific and calcareous deposits are frequently met with, and false polypi found in the heart after death.

We might expect, also, since various substances enter the circulation, that these would be observed in the Blood. Such an occurrence, however, is extremely rare. Even in Diabetes, the nicest chemical analysis can detect no sugar in the Blood.

Instances have occurred, of *air mingled with Blood*, and found in the vessels after death. Such Morgagni relates, as well from his own observations, as the testimony of Valsalva, Ruysch, and others. Though partially found in the arteries, the air abounded chiefly, it appears, in the veins.*

11. The PUTREFACTIVE PROCESS in Blood, is influenced by the state of the system. It takes place most readily in a debilitated condition:† Blood subtracted during inflammation, assumes this change later than Blood drawn during weak vascular action.

* De Sed. et Caus. Morb.—Ep. V.

† From some observations of Sir John Pringle's it appears that the *crassamentum* assumes the putrefactive change more readily than the *serum*.—7th. Paper read before the Roy. Soc.

EXP. LXV

The Blood of a very young puppy, and that of a full-grown dog, were taken when the temperature of the atmosphere was nearly the same. The former was putrid in about a quarter of the time, which elapsed before a fetid smell could be perceived in the latter.

EXP. LXVI.

A comparative experiment on the Blood of a Lamb and an Ox, taken at the same time. On the eighth succeeding day, putrefaction was evident both in the serum and crassamentum of the former, while from the Blood of the Ox, no fœtor could be perceived.

I have examined, also, the comparative tendency to putridity in the Blood of slaughtered animals, but I found it to occur in no regular relation to the tonic or atonic state. There can be, however, but little expectation of disparity in Blood received from the same hemorrhage.

Hunter ascertained, by several experiments, that the Blood of an old person becomes putrid sooner than that of the young. He bled a woman of 20, and another of 60, on the same day. The

Blood of the latter assumed this change in two days, while that of the former remained sweet till the fifth. From observations, therefore, made on the human Blood, in the tonic and atonic states, as well as from LXV. and LXVI., it is concluded, that *putrescency most readily occurs, where the Vis Vitæ is most reduced.*

Some authors, as Fernelius and Schwenke, have stated their observation of the Blood's being putrid, even, on its emission from the vein. Nay, Morton mentions from his own knowledge, the circumstance of a surgeon and his attendants fainting from the fetid smell of Blood, issuing on venesection.* Statements, however, like this, will scarcely be credited at the present day.

In two cases, I have remarked a *peculiar indisposition of the Blood to putrefaction.* The first was in Blood, taken from a subject of Pleuritis, the second of Carditis, both were females. The proportions of serum and crassamentum were natural, but in each case there was a slight fibrous tunic. At a subsequent bleeding of the latter, however, no crust was apparent, yet the Blood remained six weeks, (Dec. to Feb.) free from putridity.

* Pyretolog.

From what has been advanced on the subject of this chapter it appears that four principal *deductions* may be drawn as influencing the treatment of Disease.

1. That coagulation commences speedily in proportion to the weakness of the system, and slowly in proportion to the tonic state, and the degree of Inflammation.

2. That the firmness of the crassamentum, after the exudation of the serum, is characteristic of sthenic disease.

3. That this state is further indicated by an inordinate proportion of crassamentum; and a state of debility by an undue quantity of serum.

4. That the fibrous tunic or buff-coat, simply considered, although it occurs frequently in acute disease, is not a criterion of Inflammation, nor affords an indication for the depletory treatment.

From what has been advanced on the subject of this chapter it appears that four principal deductions may be drawn as respecting the treatment of Disease.

1. That coagulable humours especially in proportion to the weakness of the system, and

slowly in proportion to the tone of the system, and the

degree of inflammation.

2. That the firmness of the crassamentum, after

the extinction of the acrimony is characteristic of

chronic disease.

3. That this state is farther indicated by an

inordinate proportion of crassamentum; and a

state of habit by an undue quantity of serum.

4. That the fibrous tissue or buff coat simply

considered, although it occurs frequently in acute

disease, is not a criterion of inflammation, nor

affords an indication for the depletion treatment.

5. That the quantity of the buff coat is not a

criterion of the quantity of the crassamentum.

6. That the quantity of the buff coat is not a

criterion of the quantity of the serum.

7. That the quantity of the buff coat is not a

criterion of the quantity of the fibrine.

8. That the quantity of the buff coat is not a

criterion of the quantity of the albumen.

9. That the quantity of the buff coat is not a

criterion of the quantity of the casein.

APPENDIX.

CONSTITUENTS OF THE BLOOD.

(PAGE 15.)

<i>According to Berzelius.</i>		<i>According to Marcet.</i>		
<i>Soluble in Alcohol.</i>	Water	905.0	Water	900.00
	Albumen	80.0	Albumen	86.80
	Muriates of Soda & Potash	6.0	Muriates of Potash and Soda	6.60
	Lactate of Soda, and animal matter	4.0	Muco-extractive Matter....	4.00
<i>Soluble in Water only.</i>	Soda, Phosphate of		Subcarbonate of Soda.....	1.65
	Soda, and a little ani- mal Matter,	4.1	Sulphate of Potash.....	.35
	Loss	.9	Earthy Phosphates.....	.60

<i>Whitting's Analysis.</i>		<i>Bostock's Analysis.</i>	
Water	780.00	Water	80.0
Albumen	75.58	Albumen	10.0
Serosity	8.84	Uncoagulable Matter	1.0
Saline Matter	4.24	Salts	1.0
Fibrine	1.65		
Red Particles.....	131.69		

There are also Analyses of the Blood, and some observations on the contents of different Blood-Vessels, extracted from Prof. Authenrieth's Dissertations, and stated in the Edinburgh Journal for 1816.

(PAGE 31.)

SOME REMARKS ON THE SPECIFIC GRAVITY OF THE
BLOOD OF ANIMALS.

Ox, 1015·9—1053·4—1028·5—1043·8

Sheep, 1023·1—1054·3

Duck, 1109·8

Hog, 1061·0

SOLID CONTENTS BY EVAPORATION OF THE BLOOD OF
ANIMALS.

Horse, 19·3 per. cent.

Ox, 18·6—18·2

Sheep, 18·6

These observations, it is apparent, were neither in kind nor degree, sufficient for the induction of accurate conclusions.

TEMPERATURE OF CONCRETION IN THE SERUM OF OXEN
AND SHEEP.

Oxen.

Sheep.

160°—157°.....148°—142°—148°—140°

158 —156154 —140 —144

155 —148140 —142 —150

If any inference can be drawn from these observations, it is, that the Blood of Sheep coagulates at a lower temperature than that of Man. In several other animals I had regard to this subject, but as I generally found the temperature to be 150° 160°. No minutes were made of these experiments.

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OBSERVATIONS ON THE RELATIVE QUANTITIES OF SERUM
AND CRASSAMENTUM DURING HEMORRHAGE.

EXP. I.

Three glasses received the Blood of a Dog; No. 1, filled on the first gush from the wound, No. 2, two minutes afterwards, and No. 3, immediately before the death of the animal. The parcels weighed the succeeding day,

	<i>Serum.</i>	<i>Crass.</i>	
No 1, had	381.....	1272	or as 10 to 33·3
2,	153.....	473	 10 ... 30·9
3,	116.....	150	 10 ... 12·9

EXP. II.

Three cups were filled with the Blood of a slaughtered Ox; the periods similar to those of the preceding experiment; except that the 2nd was taken 3 minutes after the first effusion. On weighing the next day,

	<i>Serum.</i>	<i>Crass.</i>	
No, 1, had	430.....	1168	or as 10 to 27·1
2,	570.....	1038	 10 ... 18·2
3,	643.....	1032	 10 ... 16·0

EXP. III.

The experiment repeated. Blood weighed the day following; and the additional effusion of Serum examined, the succeeding morning.*

* When these experiments were first instituted, I weighed the Blood but once: on finding, however, that a considerable effusion of serum

Serum. Crass.

No. 1, had 497.....639 or as 10 to 12·8

2, 539.....754 10 ... 13·9

3, 618.....608 10 ... 9·8

EXP. IV.

The Experiment repeated.—The Blood weighed on the two succeeding days.

Serum. Crass.

No. 1, had 206.. ...204 or as 10 to 9·9

2, 277.....464 10 ... 16·7

3, 345.....498 10 ... 14·1

EXP. V.

Three cups of Sheep's Blood : the periods as in III. Proportions examined on the succeeding day.

Serum. Crass.

No. 1, had 350.....610 or as 10 to 17·4

2, 250.....450.....10 ... 18·0

3, 110.....450 (very soft) 10 ... 40·9

often took place, after the first had been poured off, I saw it necessary to accurate result, that the proportions should be weighed on the 1st day, and again on the 2nd or 3rd. In experiment 3, the quantity of serum was found on the *first* day to be in

No. 1, as 10 to 17·1

2, .. 10 .. 47·7

3, .. 10 .. 13·3

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EVAPORATED SERUM OF MORBID BLOOD.

No. 1.—A severe case of Pleuro-peripneumony, the solid contents were to the fluid,

12·2 per cent. at the 2d Bleeding.

10·3 3d Ditto.

No. 2.—A case of Ascites, attended by inflammatory action, (fibrous coat),

10·4 per cent.

At a subsequent bleeding (a denser fibrous coat, with cupped margin),

11·3 per cent.

No. 3.—A stout young woman, labouring under Pleuritis,

10·3 per cent.

No. 4.—In a healthy man, who suffered from high constitutional irritation, succeeding amputation,

10·2 per cent.

No. 5.—A chronic affection, where no organic disease could be detected,

7·8 per cent.

In No. 1, at the first bleeding, there was more acute disease than at the 2d; but in No. 2, the case was inversed.

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While this chapter was in the press the lamented death of Mr. Hey occurred: and I cannot refrain from paying a tribute

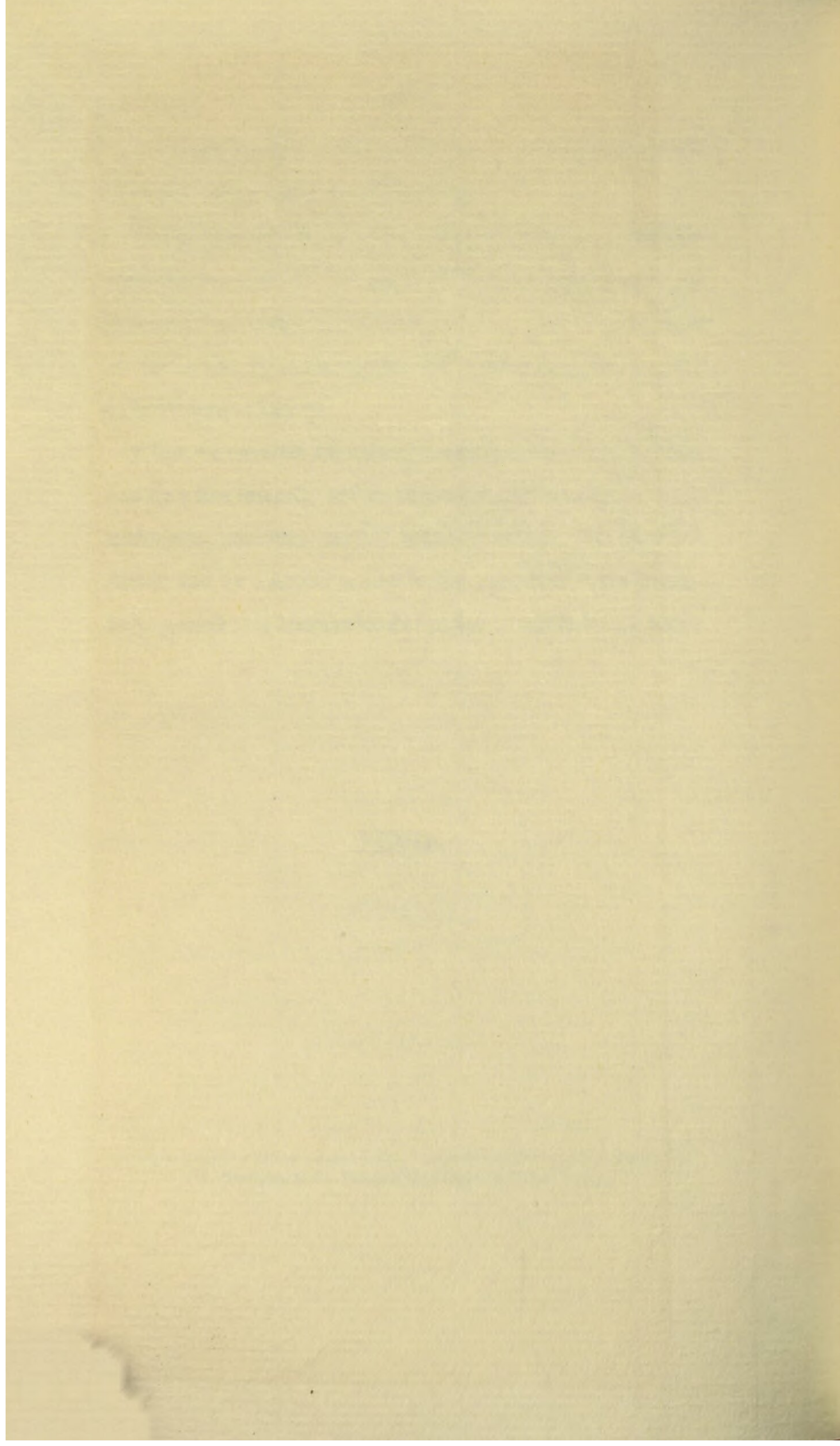
to his worth, though my feeble panegyric can neither raise his character, nor prolong its remembrance.

To his zeal for the advance of knowledge, to his eminent attainments, and to his devotedness to the interests of humanity, no ordinary eulogium is due. But to expatiate on his merits, to those who knew him were superfluous,—for to know was to admire.

While we venerate his memory, may we copy his example; and by a like integrity and steadiness of conduct, by the same undeviating attention to the discharge of our professional duties, and by a similar ardour in the pursuit of useful knowledge,—seek that honourable distinction to which he attained!

FINIS.

Bayerische
Staatsbibliothek
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Thackrah, Charles T.

An inquiry into the nature and properties of the blood, as existent in
health and disease

London 1819

Anat. 429 c

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